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American
Political
Science
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Table of Contents

ARTICLES

Dynamic Representation
James A. Stimson, Michael B. MacKuen, and Robert S. Erikson 543

Campaign Contributions and Access
David Austen-Smith 566

A Grammar of Institutions
Sue E. S. Crawford and Elinor Ostrom 582

The Advantage of Being Moderately Cooperative
Tomonori Morikawa, John M. Orbell, and Audun S. Runde 601

Thick-Skinned Liberalism: Redefining Civility
Richard C. Sinopoli 612

Mary Astell (1666–1731), Critic of Locke
Patricia Springborg 621

What To Do (and Not to Do) with Time-Series–Cross-Section Data in Comparative Politics
Nathaniel Beck and Jonathan N. Katz 634

Coalition Termination and the Strategic Timing of Parliamentary Elections
Arthur Lupia and Kaare Strøm 648

RESEARCH NOTES

Waltz, Durkheim, and International Relations: The International System as an Abnormal Form
John Barkdull 669

The Consequences of Negotiated Settlements in Civil Wars, 1945–1993
Roy Licklider 681

Issue Fluidity on the U.S. Supreme Court
Kevin T. McGuire and Barbara Palmer 691

CONTROVERSIES

The Empirical Evidence for Citizen Information and a Local Market for Public Goods
David Lowery, W. E. Lyons, and Ruth Hoogland DeHoog; Paul Teske, Mark Schneider, Michael Mintrom, and Samuel Best 705

The Enactment of Mothers' Pensions: Civic Mobilization and Agenda Setting or Benefits of the Ballot?
Cheryl Logan Sparks and Peter R. Walniuk; Theda Skocpol 710

NOTES FROM THE MANAGING EDITOR 731

BOOK REVIEWS

Political Theory 738
American Politics 756
Comparative Politics 773
International Relations 794

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ARTICLES

American Political Science Review Vol. 89, No. 3 September 1995

DYNAMIC REPRESENTATION

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If public opinion changes and then public policy responds, this is dynamic representation. Public opinion is the global policy preference of the American electorate. Policy is a diverse set of acts of elected and unelected officials. Two mechanisms of policy responsiveness are (1) elections change the government's political composition, which is then reflected in new policy and (2) policymakers calculate future (mainly electoral) implications of current public views and act accordingly (rational anticipation). We develop multiple indicators of policy activity for the House, Senate, presidency, and Supreme Court, then model policy liberalism as a joint function of the two mechanisms. For each institution separately, and also in a global analysis of "government as a whole," we find that policy responds dynamically to public opinion change. This responsiveness varies by institution, both in level and in mechanism, as would be expected from constitutional design.

How, if at all, does the force of public opinion enter into governance in America? We propose that public opinion moves meaningfully over time, that government officials sense this movement, and that—for a mixed set of motives—those officials alter their behavior in response to the sensed movement. This is dynamic representation, a simple idea and an old one. Public sentiment shifts. Political actors sense the shift. And then they alter their policy behavior at the margin. Close to our naive intuition of how representative government is to work, the story has a familiar ring. We examine the postwar United States to see whether it makes sense.

Then we turn to matters of mechanics. We consider two main ways that public preferences might get turned into policy: elections and politicians' rational anticipation of elections. We also explore the common expectation that the main institutions of the U.S. national government vary in how they serve dynamic representation.

Finally, we turn to dynamics. We ask whether the national system is efficient in turning popular sentiment into policy or whether it responds only partially. And then we see whether the government responds mainly to long-term or short-term signals about public preferences.

This dynamic representation is a macro concept. Its focus is electorates and governments, not individual voters, not individual representatives. It differs from the dominant treatments of representation in altogether ignoring the geographical dimension. Nor is it tied to legislatures as the exclusive focus of representation. Representation exists when changing preferences lead to changing policy acts. It is dynamic representation because the idea, in its essence, is structured in time.

But what is "represented" into what? At this point, one usually shifts from the general and abstract statement of the theory to some case or instance—a particular policy act or a set of particularistic measures of public opinion. We differ by measuring public opinion and policy change at a level close to the generality of representation theory. We look at (domestic) policy, not a single policy, and at opinion, not discrete opinions, all structured over time.

REPRESENTATION AS A PROCESS IN TIME

What does it mean that a government represents public feelings? Responsiveness must be a central part of any satisfactory answer. Representative governments respond to—meaning act as a consequence of—changes in public sentiment.[1] To "act as a consequence of" changes in public sentiment implies a sequence, inherently structured in time. We may say that if, by knowing about earlier changes in public sentiment, we can improve the prediction of public policy over what we could have done from knowing only the history of public policy itself,[2] then opinion causes policy, and this is dynamic representation. The demands of research design are more vexing than the simple question, Does Y follow X? But at its core, the idea is uncomplicated.

The *dynamic* character of representation has a second aspect. Most political decisions are about change or the prevention of change. Governments decide to change health care systems, to reduce environmental regulations, to develop new weapons systems, or to increase subsidies for long staple cotton growers. Or not. Thus, political decisions have a directional force to them, and their incremental character is inherently dynamic. Further, most public opinion judgments concern change as well. The public expresses preferences for "more" or "less" governmental action across different spheres: "faster school integration," "cuts in welfare spending," "getting tougher on crime," and so on. The main difference is that public sentiment is generally more vague, diffuse, than the more concrete government action.

This understanding suggests something akin to the familiar "thermostat" analogy (see, e.g., Deutsch 1963; Easton 1953; Wlezien n.d.). The public makes judgements about current public policy—most easily that government's actions need to be enhanced or trimmed back. These judgements will change as policy changes, as real-world conditions change, or as "politically colored" perceptions of policy and conditions change. And as the simple model indicates, politicians and government officials sense these changes in public judgment and act accordingly. Thus, when public policy drifts away from the public's demands for policy, the representation system acts as a control mechanism to keep policy on course.[3]

The question now is how. If public opinion governs, how does it find its way into the aggregation of acts that come to be called public policy.

THE MECHANISMS OF DYNAMIC REPRESENTATION

Start with a politician facing a policy choice. With both preferences over policy options and a continuing need to protect the electoral career from unwanted termination, the elected official will typically need to balance personal preference against electoral expediency. We presume that politicians have personal preferences for and against particular policies and also that they value reelection. Then for each choice, we can define (1) a personal ideal point in the space of policy options and (2) an *expediency point* (that position most likely to optimize future reelection chances). The expediency point might be the median voter of the relevant constituency or some similar construct. We are not concerned here about particular rules. All that matters is that the politician have a *perception* of the most expedient position.

The static choice then weighs policy preference and electoral security. Politicians create an appropriate margin of safety: those who highly value policy formulation or who feel safe at home choose policy over security; those who face competitive challenge in the next election lean toward "expediency" and security. There is a stochastic element deriving from the fact that individual actors have little agenda control; they can optimize only over *available* choices. Therefore we expect each actor to have a *distribution* of acceptable positions. While a wide range of possibilities can be envisioned, often in contemporary American politics Democrats will be arrayed with ideal points to the left of most expedient positions and Republicans similarly to the right. This is a commonplace view of practical politics.

In this static model, electoral turnover stems from events that overwhelm the margin of safety that the politicians select. Campaign finance, personal scandals, challenger tactics, the framing of electoral choice—all affect outcomes. The victims come both from those who take electoral risk by pursing policy and also from those who ignore personal preference and concentrate solely on reelection: what matters is the force of electoral events relative to the politician's expectations. Here these factors should run independent of policy preferences: conservative surprises and liberal surprises will cancel each other. In this static model, election outcomes provide little systematic influence over public policy.[4]

To breathe life into this system, let us put it into motion to see its aggregate and dynamic implications. Assume that public opinion—global attitudes toward the role of government in society—moves over time. Immediately we can expect greater turnover as the force of public opinion augments the normal electoral shocks to upset incumbent politicians' standard calculus. Now, the changes in personnel will prove systematic: rightward shifts in public opinion will replace Democrats with Republicans, and leftward shifts Republicans with Democrats. Cross-sectional representation becomes dynamic representation. Changing policy by shifting ideal points (i.e., turnover) is the standard electoral connection.

Rational Anticipation, Turnover, and Policy Consequence

Turnover from elections works most transparently with politicians who are neither well informed (until hit on the head by the club of election results) nor strategic. But that does not look at all like the politicians we observe. The oft-painted picture of members of Congress, for example, as people who read five or six daily newspapers, work 18-hour days, and leave no stone unturned in anticipating the electoral problems that might arise from policy choices does not suggest either limited information or naïveté.

We explicitly postulate the reverse of the dumb and naïve politician: (1) elected politicians are rational actors; (2) they are well informed about movements in public opinion; and (3) they agree with one another about the nature of those movements. This was said well by John Kingdon: "People in and around government sense a national mood. They are comfortable discussing its content, and believe they know when the mood shifts. The idea goes by different names. . . . But common to all . . . is the notion that a rather large number of people out in the country are thinking along certain common lines, that this national mood changes from one time to another in discernible ways, and that these changes in mood or climate have important impacts on policy agendas and policy outcomes" (1984, 153).

Postulate 1: Rationality. Political scientists tend to emphasize calculation and strategic thinking when talking about rationality. We wish to stress another dimension. Rational actors make decisions in the present, but the utility they maximize lies wholly in the future. And except as evidence for inferences about the future, the past has no importance at all.

The political import of that future orientation is that *all is anticipation.* In particular, if politicians are rational, the only elections that ought to influence their calculations are those to come in the future.

Thinking about future elections then, by postulate, is of great importance but is also extremely difficult. Politicians face a serious information problem, which becomes a desperate information problem as they push the problem out two, three, or four elections into the future. They cannot know *who* will be their opponents (an even more ugly problem as the threat of primary election challenges is added to the muddle), and they cannot know *what* issues will be raised.[5]

Postulate 2: Information. Given those difficulties, and the presumption that public policy preferences matter, what sort of information about preferences would be most valuable? Almost anything about specifics has an exceedingly low probability of being relevant. Because public preferences only rarely crystallize on specifics, this sort of information, even if relevant, is exceedingly difficult to know.

For anticipating an uncertain future, we believe trends in global sorts of preferences (e.g., whether more or less government is desirable) offer the greatest promise. At that level, public attitudes are real, knowable, and likely to be relevant to the electoral future. We postulate that politicians do attend to this information by a variety of mechanisms (personal and social, as well as formal) that are open to them.[6]

Postulate 3: Consensus. The community of politics (not only politicians but also journalists, commentators, academics, and others) talks incessantly about where public opinion is going. This talk is both face-to-face (the dialogue of the cloakroom) and public, as figures in the Washington community swap sequences of ideas with one another—for which statements and responses in *the Washington Post* or on the *McNeil-Lehrer Newshour* serve as a kind of bulletin board. The end result is a public opinion that carries clarity probably beyond its genuine ambivalent character. Were beliefs about the fundamental direction of public opinion of a random or conflicting character, then the force of opinion would be neutralized. To the degree that consensus arises on the direction of opinion change, the net effects of common beliefs can become large.

Elected politicians, we believe, sense the mood of the moment, assess its trend, and anticipate its consequence for future elections. Changes in opinion, correctly perceived, will lead politicians to revise their beliefs about future election opportunities and hazards. Revised beliefs imply also revised expedient positions. Such strategic adjustment will have two effects: (1) it will dampen turnover, the conventional path of electoral influence; and (2) it will drive policy through rational anticipation.

When politicians perceive public opinion change, they adapt their behavior to please their constituency and, accordingly, enhance their chances of reelection. Public opinion will still work through elections, however. When they are surprised by the suddenness or the magnitude of opinion change or when they are unable credibly to alter their policies, politicians, despite their best efforts, will occasionally face defeat at the polls. Rather more fitfully than was the case with dumb politicians, public preferences will operate on electoral institutions by changing the personnel and thus the aggregated preferences of elected officials.

But that is not the only public opinion effect. Changing policy from shifting perceptions of what is electorally expedient we will refer to as *rational anticipation*. In a world of savvy politicians, rational anticipation produces dynamic representation without need for actual electoral defeats.

Politicians modify their behavior at the margin. Liberals and conservatives do not change their stripes, but they do engage in strategic behavior either to minimize risk from movements adverse to their positions or to maximize electoral payoff from movements supportive of their positions. For example, in a conservative era, such as the early 1980s, conservative Democrats found it easier to break with their party and did it more often, while liberal Republicans found it more difficult and dangerous and did it less often. The result of such conditions can be substantial shifts in winning and losing coalitions without any change of personnel.

Moreover, such direct anticipation of the electoral future does not exhaust the possibilities. For other actors also anticipate the effects of future elections on the current behavior of elected officials. Those who advance policy proposals—bureaucrats, lobbyists, judges, and citizens—are concerned with what can be done successfully, be it administrative act, judicial decision, or legislative proposal. And other politicians—those who pursue a leadership role or advocate particular policies—may choose to push ahead of the curve, to multiply the effects of even marginal shifts in opinion by anticipating others' anticipated reactions.[7]

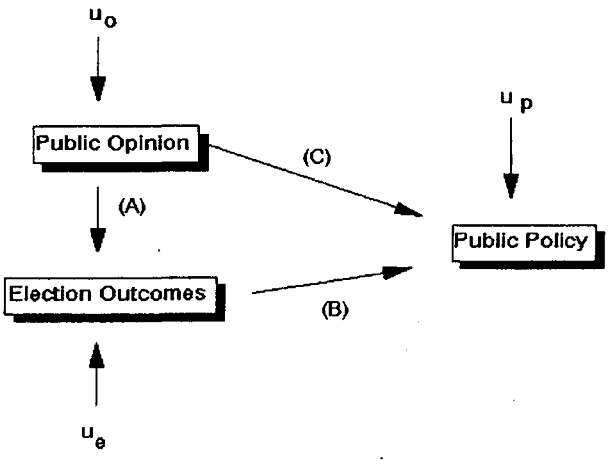
The impact of rational anticipation is thus a net shift in policy outputs from the aggregation of all these smallish strategic decisions, which (responding to the same signal) tend to move all in the same direction. It should be observable as the direct response of policy to opinion change, when election turnover effects are controlled.

A DESIGN FOR ASSESSING REPRESENTATION

This two-part setup permits three possible empirical outcomes: (1) two-stage representation may occur through the mechanism of electoral turnover, where candidate success depends upon the public opinion of the moment, which is then reflected in policy behavior; (2) movements in policy acts may reflect

FIGURE 1

The Pathways to Dynamic Representation



opinion without changes in elite personnel, the rational anticipation scheme; and (3) no representation might occur if both schemes fail. The alternatives are laid out in Figure 1. There we can see three testable linkages. The first, A, is the first stage of the electoral sequence. The question to be answered is, Does public opinion affect election outcomes? The second stage, B, is not much in doubt. Its question is no cliff-hanger: Is there a difference in policy behavior between liberals and conservatives? The third linkage, C, is rational anticipation. Its question is, Does public policy move with public opinion independently of the effects of (past) elections?[8]

Causal Ordering. Consider the possibility that we observe spurious covariation between public opinion and policymaking, which is in fact the response of opinion to policymaking. Stimson (1991), Durr (1993), and Wlezien (n.d.) each postulate a negative response of opinion to previous policymaking. How can we know that such opinion reaction is not producing the evidence we see? First, we know by design, from time ordering. We design the tests in such a way that opinion is always measured at $t - 1$, policy-making at t. This cannot rule out the possibility that opinion responds to policy, but it definitively precludes the possibility of opinion responding to current policy. Second, the opinion reaction thesis predicts negative association between policymaking and opinion; current opinion responds contrary to previous policymaking. We observe positive association between contemporary opinion and policymaking. Finally, empirical causality tests confirm our proposed ordering. A Granger analysis of opinion and a summary average of six policymaking indicators (two each from the presidency, House, and Senate) shows that opinion Granger-causes policymaking. For the proposed ordering, the key coefficient on opinion is .522 (.249) with $p = .044$. For the opposite, it is $-.062$ (.034) with $p = .086$. This evidence, limited though it is, tells us that previous opinion predicts current policy, even controlling for previous policy, whereas previous policy fails (but not by a lot) to have statistically discernible ability to predict current opinion.[9]

Enriching the Specification. The scheme of Figure 1 takes account of reality by positing other sets of causes of all phenomena as disturbances. The first, u_o, is the exogenous factors that account for changes in opinion. Not a focus of attention here (but see Durr 1993), they are such plausible forces as national optimism or pessimism arising from economic performance and reactions to past policies as experienced in daily life.

Elections are influenced by factors such as incumbent party performance (Erikson 1989, 1990; Fiorina 1981), incumbency, macropartisanship (MacKuen, Erikson, and Stimson 1989), and so forth. Those factors appear as u_e in Figure 1. And finally, u_p captures sets of causes of public policy other than representation—such things as the events and problems to which policy is response or solution. Some of these "disturbances" are amenable to modeling, and will be. Some are irreducible, and must remain unobserved.

STATISTICAL MODEL

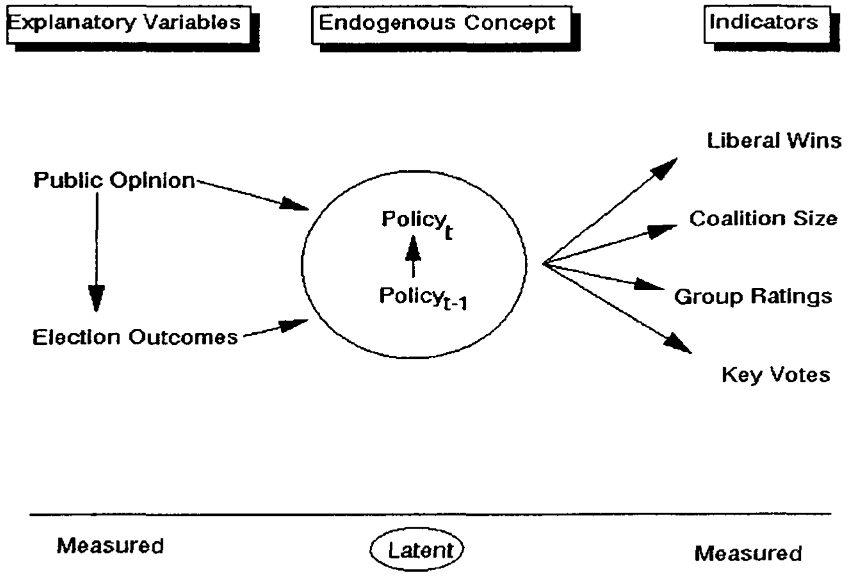
The modeling problem is simple in essence. We have information about the composition of government (Congress, the White House, and the Court) and about public opinion. There is a large array of possible indicators of the ideological tone of policymaking, our dependent variable. We wish to estimate coefficients that will represent the policymaking effects of turnover and of rational anticipation.

It is the left-hand side of the equation that presents modeling problems and opportunities. One reason our indicators of policymaking are so abundant is that no one of them is so obviously appropriate for the concept *policymaking* that we would wish to use it alone as a test of the model. We regard the ideological tone (liberalism as it is eventually to be measured) of policymaking as a latent cause of the observed policy indicators. Each of the indicators reflects some portion of that latent causation, we believe. But each also moves systematically over time as a result of forces specific to the indicator. In the terms of measurement theory, the reliable variance of each indicator has portions of both common and specific variance. And of course, all have the usual dose of stochastic fluctuation and measurement unreliability.

The estimation issues are laid out in Figure 2, a generic version of models to be estimated for the two houses of Congress. The explanatory variables (on the left) we have already discussed. Our dynamic dependent variable—the concept *policy*—is latent. We have multiple indicators of the concept, four in this illustration, which are measured variables. The estimation problem is to leverage the information in both

FIGURE 2

An Estimation Model: The Kalman Filter DYMIMIC Setup



explanatory and indicator variables (1) to estimate the latent policy concept and (2) to map its structural association with explanatory variables.

Thus we have two modeling issues, first the structural model relating exogenous variables to the latent endogenous policy concept, then the measurement issues mapping the flow from latent concept to observed (endogenous) indicators. It is possible to take on this problem in two steps—measurement, then structure—as we did in preliminary analyses. The measurement question can be understood as one of extracting the common variance from a set of indicators to produce an estimate of the latent concept. This is the usual measurement application of the family of models of factor analysis and principal components. The structural issues are then resolved by some form of time-series regression, where the standard problems are present and the standard sets of solutions are appropriate. In particular, we expect dynamics—that the climate of policymaking will show some residual influence of last year's climate, for example. Thus we need to formulate an estimation strategy that can handle dynamics.

This two-step approach (e.g., principal components followed by regression) is not, however, optimal (Beck 1990; Engle and Watson 1981; Watson and Engle 1983).[10] The measurement model can be strengthened by information from the structural analysis, and the structural analysis can be strengthened by (otherwise unused) measurement information. Thus a model that combines measurement with structural analysis is preferable. Our requirement then is a *dynamic* (DY) model with *multiple indicators* (MI) and *multiple exogenous causal variables*, a specification called DYMIMIC by Watson and Engle (1983). The challenge of the DYMIMIC specification is that the left-hand side variable (policymaking, in this instance) is both itself unobserved and a function of its unobserved past values.

The DYMIMIC specification involves a dynamic factor analysis modeled simultaneously with a more standard regressionlike structural model. It postulates a single-factor explanation of the covariance of the dependent indicators. The latent dependent variable problem is inherently nonlinear. Maximum likelihood is an appealing estimation strategy for such problems. But regression and its varied extensions have a serious shortcoming for such a specification: regression errors cannot reasonably be assumed independent of one another, an assumption necessary for maximum likelihood. Following the lead of Watson and Engle (1983) and Beck (1990), we turn to the Kalman filter as a device for the application of maximum likelihood to this otherwise intractable problem.

The Kalman filter is a linear model, like regression. It differs in approach to the time-series case. Regression uses all sample information—time points *1* though *T*—about X and Y to predict all individual values of Y. The Kalman filter, in contrast, proceeds recursively through time. It too makes a prediction for each Y_t. But unlike regression, that prediction is based only upon information available before t. Thus all parameters are estimated T times—on every possible subsample from times *1* to T. The advantage to all this computational tedium is that the Kalman filter errors, the difference between Y_t and the Y_t predicted, conditional on information about X and Y through $t - 1$, are mutually independent. With an assumption of normally distributed errors, we can use maximum likelihood to solve this otherwise intractable problem in nonlinear dynamic estimation.[11]

The Kalman filter consists of a "transition" model, which shows the structural association among exogenous variables and the latent dependent concept (called the *state* variable) and a set of measurement models (one for each dependent indicator) that map the estimated state variable onto the observed indicator. Information from the transition model may be interpreted roughly as if the model were a dynamic regression. Information from the measurement models is of two kinds: slope and intercept analogues that relate the scale of the latent concept to the varied scales and metrics of the indicators[12] and correlations between state variable and indicators, whose squares become estimated communalities. The former are of limited theoretical interest, because differences of scale render them noncomparable. The latter is interpreted as the valid variance of the indicator.

Linear regression presents an obvious alternative to the Kalman filter for estimating the relationships of interest. It can estimate only a single left-hand-side variable at a time, and its error assumptions are often troublesome for the dynamic case. It has, nonetheless, the virtue of familiarity. As a check on the theoretically superior but unfamiliar Kalman filter estimations, we redo the major analyses with dynamic regressions in the Appendix. Selecting a single endogenous indicator from each table to follow, we repeat the major tests for dynamic representation as straightforward lagged variable regressions (see Appendix A). The regression analyses support the same inferences as the Kalman filter estimations in all cases.

MEASUREMENT

The raw materials of dynamic representation are familiar stuff: public opinion, elections, and public policy together form the focus of a major proportion of our scholarly activity. But familiar as these concepts are, longitudinal measures of them are (excepting elections) ad hoc at best and more often nonexistent. It is easy to think of movements of public opinion over time and public policy over time. It is not easy to quantify them. The situation—familiar concepts but novel measures—requires more than the usual cursory attention to measurement concerns. We begin with public opinion.

The Measures: Public Opinion and Elections

To tap public opinion over time we have the measure DOMESTIC POLICY MOOD (Stimson 1991).[13] Mood is the major dimension underlying expressed preferences over policy alternatives in the survey research record. It is properly interpreted as left versus right—more specifically, as global preferences for a larger, more active federal government as opposed to a smaller, more passive one across the sphere of all domestic policy controversies.[14] Thus our public opinion measure represents the public's sense of whether the political "temperature" is too hot or too cold, whether government is too active or not active enough. The policy status quo is the baseline, either explicit or implicit, in most survey questions. What the questions (and the mood measure) tap then is relative preference—the preferred direction of policy change.

Displayed in Figure 3, the POLICY MOOD series portrays an American public opinion that moves slowly back and forth from left (up on the scale) to right (down) over time and is roughly in accord with popular depictions of the eras of modern American politics. It reaches a liberal high point in the early 1960s, meanders mainly in the liberal end of its range through the middle 1970s, moves quite dramatically toward conservatism approaching 1980, and then begins a gradual return to liberalism over the 1980s. Note as well that the neutral point (50% liberal, 50% conservative) means something: points above 50 mean that the public demands more liberal policy, points below 50 mean that the public wants more conservative policy. Thus, while the public's conservatism peaked in 1980, the public continued to demand more conservative policy (though by smaller margins) until 1984. (Thus we may think of our mood measure as a signal to politicians about the intensity and the direction of political pressure. It represents a demand for change.)

As instruments for the partisan drift of elections, we employ measures of partisan and ideological composition. Varying by focus of analysis, these instruments include factors such as the party of the president and the Democratic percentages of the House of Representatives and the Senate. These latter numbers measure seat turnover in the two houses (which follow different paths) and thus capture what is probably the main carrier of the electoral message—the introduction and disappearance of personnel in the body. Real changes in real people clearly indicate changes in the houses' policy preferences. Further, they also hammer home the partisan electoral message to incumbents who, cleverly or otherwise, escaped electoral judgement.

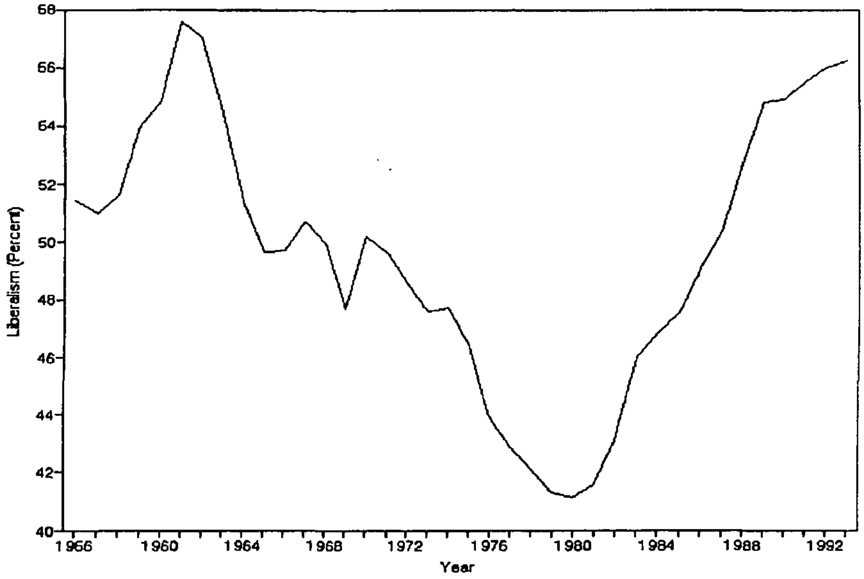
The Measures: Policy Change

What is policy liberalism, and how can we measure it? What we observe is decisions such as congressional votes—not quite "policy." Our view is that each involves policy *change* at the margin. The issue as it is typically confronted is, Should we move current government policy in more liberal (expansive) directions or in more conservative ones? What we observe is who votes how. We see, for example, a particular vote in which the liberal forces triumph over conservative opponents. We take such a vote to mean that in fact the (unobserved) content of the vote moves policy in a liberal direction—or resists movement in the conservative direction.

This is a direct analogy to public opinion as we measure it. We ask the public whether government should "do more" or "spend more" toward some particular purpose. We take the response, "do more," "do less," "do about the same" to indicate the

FIGURE 3

Public Opinion over Time: Domestic Policy Mood, 1956–93



preferred direction of policy *change*. In both cases direction of change from the status quo is the issue.

Measuring this net liberalism or conservatism of global policy output seems easy enough in concept. We talk about some Congresses being more or less liberal than others as if we knew what that meant. But if we ask how we know, where those intuitions come from, the answer is likely to be nonspecific. The intuitions probably arise from fuzzy processing of multiple indicators of, for example, congressional action. And if none of them by itself is probably "the" defensible measure, our intuitions are probably correct in netting out the sum of many of them, all moving in the same direction. That, at least, is our strategy here. We will exploit several indicators of annual congressional policy output, each by itself dubious. But when they run in tandem with one another, the set will seem much more secure than its members.

Congressional Rating Scales. Rating scales are a starting point. Intended to tap the policy behaviors of individual House members and Senators, scales produced by groups such as Americans for Democratic Action (ADA) and Americans for Constitutional Action (ACA), later American Conservative Union (ACU), are now available for most of the period in question. Neither of these is intended to be a longitudinal measure of congressional action; and from a priori consideration of the properties such a measure would want, this is not how we would derive one.[15] But if scales move similarly across chambers and scales from different organizations move in common over time, then we begin to believe that whatever it is they are measuring is probably global liberalism or conservatism of roll-call voting. Thus, as a measure of NET GROUP RATING, we take the yearly average of the House's (or Senate's) ADA score and (100 minus) the ACA/ACU score.

Congressional Roll-Call Outcomes. The strength of the rating scales is their cross-sectional validity: they discriminate liberals from moderates from conservatives in any given year. Their weakness is longitudinal validity: we are less confident that they discriminate liberal from moderate from conservative Congresses. For greater face validity, we turn to the roll calls themselves as measures of policymaking. A quite direct measure is the answer to the questions, On ideological votes, who wins? and By how much do they win? Provided that we can isolate a set of roll calls that polarize votes along the left-versus-right main dimension of American domestic politics, measuring the degree of, say, liberalism is as easy as counting the votes. If we know which position on the vote is liberal and which conservative, then all that remains is to observe who won and by how much (and then aggregate that roll-call information to the session).

We exploit the cross-sectional strength of the rating scales (specifically, ADA) to classify roll calls. For each of the 25,492 roll-call votes in both houses for 1956–90, we classify the vote as left-right polarized or not (and then in which direction). The criterion for the classification as polarized is that the vote must show a greater association with member ADA scores than a hypothetical party-line vote for the particular members of each Congress. The intuition of this criterion is that we know we are observing a left-right cleavage when defection from party lines is itself along left-right lines—conservative Democrats voting with Republicans, liberal Republicans voting with Democrats. Although the party vote itself might be ideological, we cannot know that it is.[16] One measure of the net liberalism of a session (for each house separately) is then simply the median size of the liberal coalition (on votes where the liberal and conservative sides are defined). A second approach to the same raw data is to focus on winning and losing, rather than coalition size.[17] In this set of measures we simply count the percentage of liberal wins. We are observing quite directly then who wins, who loses, and by how much.

The Dramatic Acts of Congress: Key Votes. Scales of roll-call votes tell us about the overall tenor of public policy. Probably an excellent basis for inference about the net direction of policy movement, they do not distinguish between minor matters and those of enormous public consequence and visibility. Getting a good measure of "importance" presents a formidable challenge, requiring great numbers of subtle judgments about content and context. It is nonetheless desirable to have some indication of whether legislative activity produces something of import. A particular subset of legislation, the *Congressional Quarterly* "key votes" for each session of Congress, does attempt to distinguish the crucial from the trivial. The virtues of this set of votes are that it reflects the wisdom of expert observers of Congress at the time about what was important, and the measures are readily coded into liberal or conservative actions (and some that are neither).[18]

We quantify the key votes as a combination of who wins and by how much. Accordingly, we average (1) the percentage of liberal wins and (2) the size of the liberal winning coalition. Crude, the measures nonetheless tap the issue in question, the direction of highly visible outcomes. The resulting time series are noisy (as would be expected from the small numbers of votes for each year), evincing a good deal of year-to-year fluctuation that seems meaningless. But they also show a picture of episodes of major policy change occurring exactly when expected for the Great Society (liberalism, peaking in 1965) and the Reagan Revolution (conservatism, peaking in 1981) periods respectively.

To get a sense of how legislative policy has moved over the years, look at Figure 4. (Figure 4a) presents our four measures for the House of Representatives. (To keep the eye on systematic movement, we have smoothed the graphs by taking a centered three-year moving average for each series. Note that we smooth only in this graph: we use the measured data for the statistical analysis.) It is clear that each indicator (wins, coalition size, ADA–ACA ratings, and key votes) contains both a common component and an idiosyncratic component. The lines move together, with a bit of zig and zag around the main flow. The panel for the Senate (Figure 4b) carries a similar message. Peaks of liberalism came during the early 1960s and late 1980s, with conservatism at its height around 1980. While thus similar in outline, the patterns are not quite identical.

Presidential Policy Liberalism. The beginning point of dealing with the presidency is noting the near impossibility of direct measures of presidential liberalism from what presidents say and do. While we have an intuition about various acts and speeches, any attempt to quantify that intuition, to extract acts from the context of actions, quickly becomes hopelessly subjective. The alternative is to look instead at presidents through their quantifiable records of interacting with the legislature and judiciary.

We know how often particular members of Congress support and oppose the president. And we can measure the liberalism of individual members in several ways. The most convenient of these is ADA scores, which are present for the entire period, as other comparable indicators are not. And we know that ADA ratings are very highly correlated with other ratings when available—positively or negatively—so that they can serve as a useful instrument of the underlying concept.

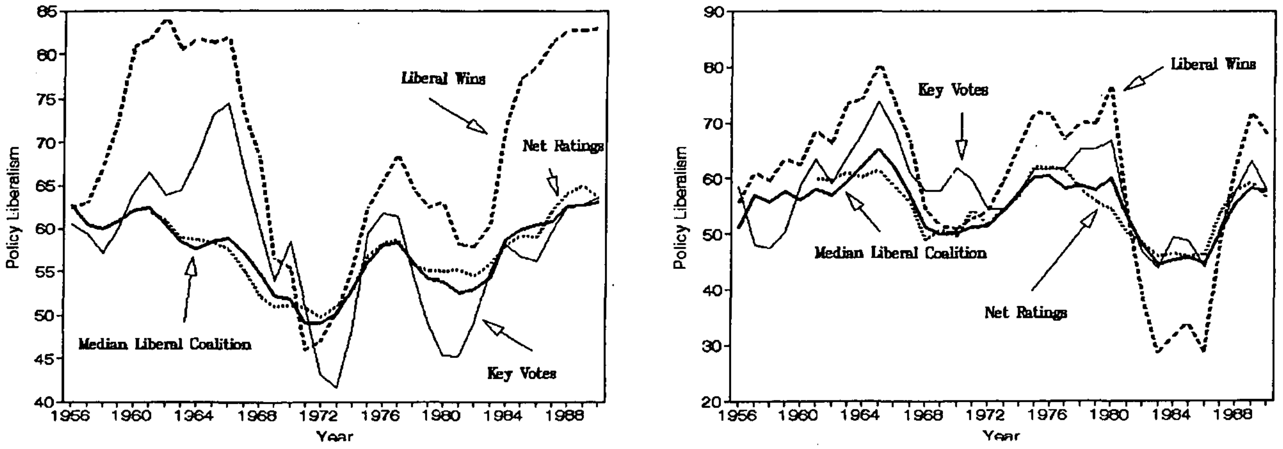
How then to combine these different pieces of information? A first approach is to ask the question, How liberal are the regular supporters of the president each year?, and then adopt that standard as a reflection of what the president wanted from Congress. That, however, is confounded by shared partisanship between president and member. We expect members of the president's party to be more likely to be regular supporters—independent of ideological agreement with the president's program. To deal with shared party ties as a confounding factor in presidential support, we opt instead to focus on presidential support within party. The strategy is first to divide each party into support and opposition groups based upon whether member presidential support is above or below the average for the party. The mean ADA rating of each party's "support" group is then an estimate of the president's ideological position. The opposition groups similarly measure the reverse. The measurement question then may be reduced to how such separate estimates are to be combined. For a summary measure of presidential position we perform a principal components analysis of the eight indicators (*support* vs. *oppose*, by party, by house). That analysis shows decisively that each of the eight taps a single underlying dimension. Such a dimension is estimated with a factor score and rescaled (to mean 50, standard deviation 10) to approximate the ADA scales from which it was derived.

For a second legislative presidential position measure we simply take the recorded presidential position for the key votes and compute the percentage of presidential stands each year that are liberal, where again the votes are classified by polarization with individual ADA ratings.

Presidential Interaction with the Court. With less regularity and on a quite different set of issues, presidents make their policy views known to the U.S. Supreme

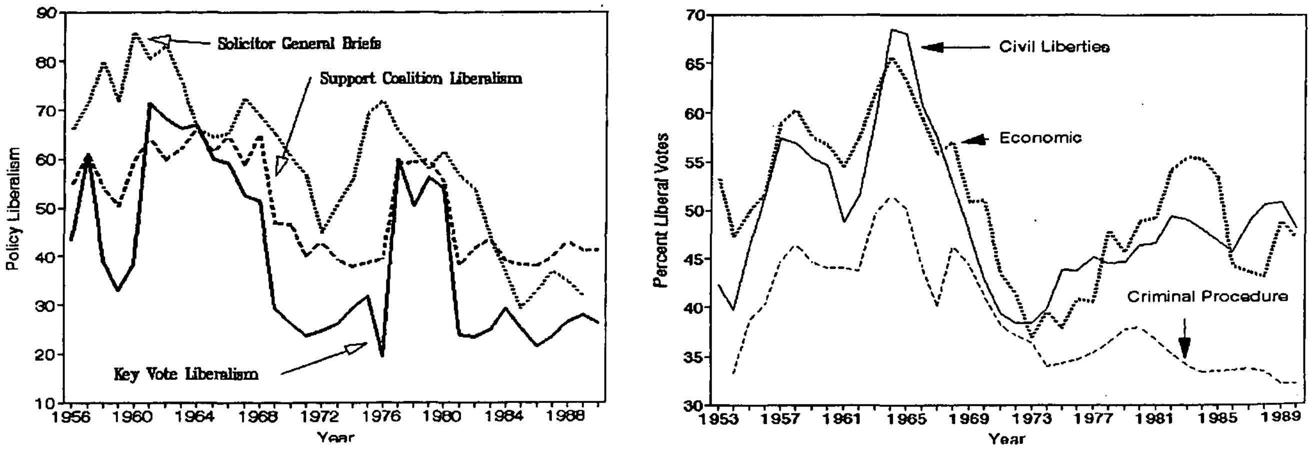
FIGURE 4

Indicators of Public Policy Change in Four Parts of American Government (Three Year Moving Averages)



a. The House of Representatives

b. The U.S. Senate



c. The Presidency

d. The Supreme Court

Court. The mechanism for doing so formally is the amicus curiae brief filed by the presidency's designated agent to the courts, the solicitor general (Lee 1986; Segal 1988; Salokar 1992). On over 700 occasions in the 1953–89 terms, the solicitor general went on record with the Court, arguing that the holdings of particular judicial decisions ought to be affirmed or reversed. About 90% of these briefs take positions on cases that are themselves classifiably liberal or conservative.

We employ the solicitor general briefs data as leverage to measure presumed presidential position on judicial issues. Using the direction coding from the Spaeth Supreme Court data base for the case and our knowledge of whether the solicitor general argued to affirm or reverse,[19] we code each of the briefs as to direction—liberal, conservative, or nonideological. It is then an easy matter to produce aggregated annual scales as percentage liberal of the ideological positions taken.

A quick comparison of the presidential series with the legislative series (in Figure 4) suggests less coherence in the presidential measures. Much of the discord comes from the SOLICITOR GENERAL series (which we retain, nevertheless, for its substantive value). Note also that the presidential series is typically more conservative than the two congressional series, as we might reasonably expect from the historical party control of the two institutions.

Supreme Court Liberalism. For data we have the Supreme Court data base for the period 1953–90. From that, we can content-classify the majority position in individual cases as liberal, conservative, or neither; and from that, the lifetime liberalism or conservatism of individual justices is readily derived. Then we return to the individual cases and scale the majority and dissenting votes by the justices who cast them. This allows a content-free second classification of the majority position as liberal, conservative, or not ideological.[20] From this we build annual measures of the major-case content categories. We have chosen four such categories—CIVIL RIGHTS AND LIBERTIES, CRIMINAL PROCEDURE, ECONOMICS, and OTHER[21]—the number a compromise between separating matters which might in principle produce different alignments and grouping broadly enough to have sufficient cases in each for reliable annual measures.

For each measure we construct a time series consisting of the percentage of all votes cast by the justices on the liberal side of the issue, whichever that is, for the year. This focus on justice decisions, rather than aggregate outcomes of the Court, appears to produce a more moderate measure over time than the alternative. This is in the spirit of Baum (1988), who argues that the liberalism of the Warren Court and the conservatism of the Burger Court both tend to be overestimated from outcome measures (i.e., which side won or lost).

We examine the first three domains in Figure 4.[22] There we see that the issue domains move pretty much in tandem. All domains show the famous liberalism of the Warren Court in the mid-1960s and the conservative reaction of the Burger Court. Most show a modest rebound of liberalism in the early 1980s, which then reverses from the influence of new Reagan justices.

The pattern of more substantive notice is that the "CRIMINAL PROCEDURE" cases produce no liberal rebound in the 1980s. This is an interesting exception, for public attitudes toward crime and criminals are themselves an exception to the growing liberalism of the 1980s (Stimson 1991). This is a case where the conservative message ("The solution is more punitive law enforcement") is still dominant.

THE EVIDENCE FOR DYNAMIC REPRESENTATION

Representation, from our perspective, works on all governing institutions. If we were more adept at measurement, for example, the national bureaucracy could be a focus for analysis. We do have decent indicators for the presidency, each of the houses of Congress, and the Supreme Court. We take them up separately. We begin our representation analysis with the presidency.

The Presidency

Presidential policy positions contain a bit of subtlety around a pattern of brute simplicity. The latter is that presidential position varies to an extraordinary degree with presidential party: Democrats are liberal, Republicans conservative. This is not the sort of pattern that merits much modeling attention. Our goal here is to model out the brute simplicity (with a presidential party dummy variable) in order to see the subtlety. That allows us to observe variations within party and year-to-year variations within a presidency.

Analytic Design. In Table 1 and those to come our analysis will be in three parts and use two methods. The problem at hand is to estimate the impact of opinion on policy and then to apportion that impact into composition and rational anticipation linkages. Thus for each analysis we undertake three steps. First, we ask the question, Does opinion move policy? To answer it, we estimate a reduced-form equation in which the endogenous effects of elections are left out of the model. The estimation technology is the Kalman filter setup, with latent dependent variable and multiple dependent indicators. Second, we ask, Does opinion influence election outcomes?, and estimate the opinion-elections link to answer the question. Here we model the effect of opinion on changing the personnel of government. With a single dependent variable (e.g., percentage of House seats Democratic), dynamic regression is the technique of choice. Third, we ask, Through what mechanisms does opinion work on policy?, and estimate a full model of representation in which both opinion and the endogenous composition variable work on policy outcomes. Again we employ the Kalman filter setup to model a latent-variable-multiple-indicator setup. This provides estimates of the composition to policy link and, most important to us, the direct effect of opinion on policy (rational anticipation).

We begin by examining the simplest (but perhaps the most fundamental) relationship. Table 1, column 1, shows the reduced-form equation, where we model PRESIDENTIAL POLICY LIBERALISM on previous POLICY LIBERALISM[23] and on PUBLIC OPINION (at $t - 1$ in this policy equation and most subsequent policy analyses). Here we pay no attention to process but instead ask the question, Is PRESIDENTIAL LIBERALISM associated with opinion change by whatever route? The answer, clearly, is *yes*. Here we see that each point change in opinion produces about .49 points in PRESIDENTIAL LIBERALISM.

We know from earlier analyses (Stimson 1991) that presidential elections turn, in part, on public opinion (measured by policy mood). A skeletal version of that work appears in an OLS regression reproduced in Table 1, column 2. Here the dependent variable is simply whether the elected president is Democratic (or Republican), and the two independent variables are MACROPARTISANSHIP (the Democratic percentage of partisans) and PUBLIC OPINION (very roughly, the percentage liberal on a "typical" policy question, at the time of election.) We expect MACROPARTISANSHIP and PUBLIC OPINION to be both theoretically and statistically important in determining which party captures the presidency—and they are.[24]

Given the party differences in presidential behavior, this electoral connection itself produces representation of a sort. Here we wish to pursue the more subtle issue of public opinion influence on the presidency beyond the elections that produced the incumbents. In the analyses to follow presidential party captures the electoral turnover route to representation and PUBLIC OPINION represents the rational anticipation effect. Also included is the usual dynamic term (PRESIDENTIAL LIBERALISM of the previous year). The estimation also produces measurement models for the congressional KEY VOTE LIBERALISM, the PRES-

TABLE 1

Estimating the Links to Presidential Representation: Three Structural Models with Coefficients and Standard Errors (in Parentheses)

	Model and Estimator		
Variables	Reduced Form (Kalman Filter)	Presidential Party (Regression)	Full Model (Kalman Filter)
DYNAMICS (Y_{t-1})	.764	—	.192
	(.121)		(.081)
PUBLIC OPINION$_{t-1}$	.490	.061[a]	.741
	(.216)	(.027)	(.273)
PRESIDENTIAL PARTY	—	—	24.360
			(2.319)
MACROPARTISANSHIP	—	.098	—
		(.028)	
Intercept	−21.428	−9.510	−22.403
	(11.879)	(2.855)	(16.578)
Number of cases	35	9	35
Adjusted R^2	—	.583	—
Measurement model (communalities)			
KEY VOTE LIBERALISM	.900	—	.988
PRESIDENTIAL SUPPORT COALITION	.994	—	.911
SOLICITOR GENERAL AMICUS BRIEFS	.381	—	.346

Note: In this and the tables to follow, column 1 is a Kalman filter estimation of the simple effect of public opinion on presidential policy, column 2 estimates the public-opinion-to-elections link with linear regression, and column 3, a Kalman filter estimation, models policy as a function both of party and of public opinion directly.

[a] The prediction of presidential party uses contemporaneous public opinion, not lagged.

IDENTIAL SUPPORT COALITION series, and the SOLICITOR GENERAL AMICUS BRIEF series.

The result, in Table 1, column 3, provides support for the obvious party effect: Democratic presidents are about 24 points more liberal than Republicans. Of more interest, the coefficient estimated for PUBLIC OPINION shows a quite substantial effect, even after controlling for presidential party. Because their empirical ranges are nearly the same, the relationship between OPINION and PRESIDENTIAL LIBERALISM (estimated in the Kalman "State" variable) can be summarized simply: each point movement in PUBLIC OPINION produces about .74 points movement in presidential policy position. That discriminates both between presidents (Eisenhower among Republicans, and Kennedy and Johnson among Democrats are more liberal than their successors) and within terms.

The measurement analysis in Table 1 provides the communalities of the indicators.[25] They provide a rough idea of the overall "fit" of the model to the empirical measures. Here, the communalities (col. 3: .99, .91, and .35) suggest that the indicators of presidential liberalism derived from the president's interactions with Congress are closely tied to the latent concept, the SOLICITOR GENERAL series less so. That is an expected result because the two congressional indicators of presidential position are more reliably estimated than the judicial materials permit. This should not obscure the genuinely strong association between estimated presidential liberalism and the judicial positions scale.

The House of Representatives

The House of Representatives is by name and constitutional tradition an obviously appropriate target for our representation theory. Contemporary conventional wisdom, however, suggests that modern practices might make the House less than fully responsive to public opinion pressure. It is widely believed (particularly by those who propose to cure the disease with term limitations) that the modern House is insulated beyond the public's ability to move it. (For thoughtful discussion on this matter, see Fiorina 1989 or Jacobson 1991.)

The first-cut analysis (see Table 2, col. 1)—wholly inconsistent with the conventional wisdom—confirms representation for the House.[26] PUBLIC OPINION shows up as statistically significant, with a coefficient indicating a translation (.85) from changes in PUBLIC OPINION to public policy. That is to say, every percentage change in public opinion liberalism produces about eight-tenths of a point change in House policy liberalism. PUBLIC OPINION matters for House policy: the question is how.

Given the well-developed reelection skills of its members, the first linkage, that between public opinion and electoral control over the House, might be expected to be attenuated. Our straightforward, conventional model (see Table 2, col. 2) for such a linkage produces little support for an opinion-election connection in the House of Representatives. Clearly, MACROPARTISANSHIP matters, and so does the standard midterm loss for the president's party. However, PUBLIC OPINION falls short of statistical significance (t = 1.715): we cannot say that policy preferences affect the House elections.[27]

We may then ask how public opinion affects House policy. A clear alternative arises from understanding that House members may be so adept at reelection because they practice rational anticipation so well. When we estimate a full model that includes both PERCENTAGE DEMOCRATIC SEATS in the House and PUBLIC OPINION, we see both highly significant. Each additional percentage Democrat (i.e., about 4 House members) produces about .48 percent liberalism in policy; each point shift in PUBLIC OPINION produces an additional (and distinct) 1.01 points shift in policy. Note two things. First, the election turnover force on policy is only marginally connected to public preferences. (It represents shifts in partisanship, midterm effects, and other factors.) Second and most important, the rational anticipation connection is very powerful. It produces a direct, one-to-one connection be-

TABLE 2

Estimating the Links to House Representation: Three Structural Models with Coefficients and Standard Errors (in Parentheses)

	Model and Estimator		
Variables	Reduced form (Kalman Filter)	Democratic Seat % (Regression)	Full Model (Kalman Filter)
DYNAMICS (Y_{t-1})	.311	.490	.230
	(.124)	(.257)	(.109)
PUBLIC OPINION$_{t-1}$	.850	.352[a]	1.010
	(.226)	(.205)	(.249)
PERCENT DEMOCRATIC	—	—	.478
			(.230)
CUMULATIVE VIETNAM DEATHS	−.468	—	−.488
	(.094)		(.089)
MIDTERM ELECTION[b]	—	−4.928	—
		(1.917)	
MACROPARTISANSHIP	—	.635	—
		(.247)	
Intercept	−2.026	−30.904	−34.712
	(9.946)	(26.876)	(21.648)
Number of cases	35	18	35
Adjusted R^2	—	.354	—
Measurement model (communalities)			
% LIBERAL WINS	.881	—	.911
MEDIAN LIBERAL COALITION SIZE	.941	—	.922
NET GROUP RATING[c]	.834	—	.795
KEY VOTE AVERAGE[d]	.511	—	.530

[a] The prediction of election uses contemporaneous public opinion, not lagged.
[b] Scored +1 for Democratic administrations at midterm, −1 for Republican administrations at midterm, and 0 if not midterm.
[c] Scored as average of ADA and ACA/ACU rating reflected in the liberal direction.
[d] Average of % key vote liberal wins and key vote median liberal coalition size.

tween percentage shifts in PUBLIC OPINION and percentage shifts in public policy. House members do not wait for elections to hear the public's message; they anticipate it and produce the appropriate response.

The measurement results in this case again suggest variation in indicator validity. The key vote indicator shows the lowest association with the latent concept. That is an expected result of the measure's lesser reliability, a matter of extracting estimates from 15–20 votes per year rather than the 100–1,000 that contribute to other measures. Again, we retain it for its substantive meaning.

We have also entertained models (for both House and Senate policy change) in which the president plays an agenda-setting role. These become complicated by the fact that there are (at least) two offsetting effects of presidential policy leadership. We expect, for example, a Democratic president to produce a more liberal agenda. But a rigorously liberal presidential agenda also makes it likely that liberals will win less often (because the proposals are tougher) and by smaller majorities. Including presidential party in the models shows these confounding patterns, with usually small and nonsignificant effects. We do not report them, because we believe they would be improperly interpreted. A valid assessment of presidential agenda setting would require measures of the content of the presidential agenda, which is well beyond our scope here.

The Senate

By some standards, the Senate should look very different. The original constitutional design (and schoolbooks) expect that the Senate should act more deliberately than the House; it should react less directly to public opinion. Yet political reality emphasizes that senators, compared to their House colleagues, enjoy only a very modest reelection advantage. Having their careers thus more exposed to public opinion shifts, practically minded senators should be especially attentive to the public's policy preferences.

The straightforward reduced-form version of the test (in Table 3, col. 1) shows a clear and strong relationship between PUBLIC OPINION and Senate policymaking. The translation of opinion into policy is even greater than a similar view of the House: every point change in public preferences yields 1.18 points in policy. Thus the Senate clearly reflects public opinion. We next ask, By what route does this force penetrate the institution?

As conventional wisdom expects, the Senate exceeds the other elected branches of the national government in its *electoral* responsiveness to public opinion. The minimal model of Senate elections (see Table 3, col. 2) shows a very substantial and direct link between PUBLIC OPINION and Senate partisan control. Every point in PUBLIC OPINION is worth about one Senate seat (1.022). In comparative terms, the Senate's partisan balance is much more sensitive to policy preferences than is the House's (note the coefficients of 1.02 and .35 for the Senate and House).[28] Clearly, the Senate is the institution for which election outcomes matter most directly.

Happily for dynamic representation, this strong electoral connection produces a corresponding influence on public policy. The Senate policy model (in Table 3, col. 3) indicates that every new Senator produces about 1.17 points in policy change. Notice also that when we control for the electoral connection, we virtually eliminate the statistical evidence for a public opinion effect. Thus, we conclude that the Senate works like a textbook representation mechanism: Senate elections are responsive to public opinion, and then the new membership produces expected policy outcomes. Electoral turnover dominates the rational anticipation connection.

TABLE 3

Estimating the Links to Senate Representation: Three Structural Models with Coefficients and Standard Errors (in Parentheses)

	Model and Estimator		
Variables	Reduced Form (Kalman Filter)	Democratic Seats (Regression)	Full Model (Kalman Filter)
DYNAMICS (Y_{t-1})	.401 (.130)	.194 (.217)	.161 (.133)
PUBLIC OPINION$_{t-1}$	1.183 (.367)	1.022 (.265)	.443 (.389)
PERCENT DEMOCRATIC	—	—	1.174 (.319)
MACROPARTISANSHIP	—	.818 (.391)	—
Intercept	−37.777 (22.235)	−67.691 (27.808)	−43.622 (22.489)
Number of cases	35	18	35
Adjusted R^2	—	.715	—
Measurement model (communalities)			
PERCENT LIBERAL WINS	.997	—	.999
MEDIAN LIBERAL COALITION SIZE	.926	—	.911
NET GROUP RATINGS[a]	.700	—	.687
KEY VOTE AVERAGE[b]	.563	—	.564

[a] Scored as average of ADA and ACA/ACU rating reflected in the liberal direction.

[b] Average of % key vote liberal wins and key vote median liberal coalition size.

The Supreme Court

Faithfully representing public opinion is no part of the mandate of the Supreme Court of the United States. The Court is to be governed by the Constitution and the law but not by public opinion. Scholars have, however, long suspected otherwise (Adamany 1973; Barnum 1985; Caldeira and Gibson 1992; Casper 1976; Dahl 1957; Funston 1975; Gates 1987; Marshall 1989; Mishler and Sheehan 1993). For the Court deals in public business and is made up in the main of men and women whose careers have led them to be sensitive to how the public wants its business done.

Once on the Court, justices will also have reason to monitor public opinion. Again, we see two main reasons. First, justices care deeply about substantive outcomes. They also share policymaking authority with elected politicians. When competing with politicians, justices must consider the possibility that their decisions will be overridden or indifferently enforced. They compare the policy outcome that obtains when they choose their "ideal point" and engage political opposition against the outcome likely when they compromise in an effort to avoid active political opposition. Thus justices who wish to exert authority over the direction of American life will anticipate actions of the other branches of government. Further, institutionally minded justices will want to avoid public defeat and the accompanying weakening of the Court's implicit authority: they will compromise in order to save the institution (Murphy 1964). All this implies paying some attention to what the public wants from government. But "paying some attention" is some distance from the immediate concern public opinion demands of elected officials. And so the representation we expect of the Court is respect for the general contours of public expectation but not the omnipresent finger in the wind we expect of the elected branches.

As with the other branches, we begin our look at the Court with the basic representation question, Do Court decisions reflect changes in public opinion? The first-order question is answered by the first equation of Table 4. Court decisions do, in fact, vary in accord with current public preferences. Every point in PUBLIC OPINION shift produces a (significant) movement of .30 points in Court policy. To the extent that the measures are comparable, this dynamic representation is about a third to a quarter as effective as for the House and Senate. Of course, the next question is how that public opinion gets translated into the Court.

TABLE 4

Estimating the Links to Supreme Court Representation: Three Structural Models with Coefficients and Standard Errors (in Parentheses)

	Model and Estimator		
Variables	Reduced Form (Kalman Filter)	Court Composition[a] (Regression)	Full Model (Kalman Filter)
DYNAMICS (Y_{t-1})	.651 (.159)	.929 (.048)	.567 (.159)
PUBLIC OPINION$_{t-1}$	.302 (.125)	.115 (.079)	.171 (.150)
COURT COMPOSITION[a]	—	—	.132 (.104)
PRESIDENTIAL PARTY dummy (Democrat = 1)	—	1.442 (.722)	—
Intercept	−1.505	−4.429 (4.386)	3.935 (7.510)
Number of cases	35	37	35
Adjusted R^2	—	.941	—
Measurement model (communalities)			
CIVIL LIBERTIES	.899	—	.880
CRIMINAL PROCEDURE	.513	—	.527
ECONOMIC	.728	—	.751

[a] Justice lifetime liberalism.

Again, there exist two main channels: composition and rational anticipation. The composition mechanism is not directly electoral, of course, but instead reflects a blend of congressional and presidential politics. When presidents decide whom to nominate and Senates decide whom to confirm, both will reflect their own policy preferences (their ideal points) and also their sense of what the public will accept. Thus the nomination-confirmation process embodies the electoral and the rational anticipation mechanisms built into the presidency and the Senate. We approximate this composite in the equation reported in Table 4, column 2. Here we find that the composition of the court reflects both the party of the president (1.44 points) and current public preferences (.12 points). Both are of substantively important magnitude and in the predicted direction, but neither is quite significant by the conventional (.05) criterion.

Thus it is with theoretically limited expectations that we model the Court's rational anticipation of public opinion. The full-model specification (Table 4, col. 3) shows that both composition and rational anticipation produce correctly signed but statistically weak effects. Given the collinearity between the series, we cannot distinguish which pathway carries the most weight. In dynamic representation terms, the Court evinces modest—but only modest—responses to changes in public preferences.

Commenting on Mishler and Sheehan 1993, Norpoth and Segal (1994) argue that the role of public opinion on Court decisions is wholly indirect through the election-nomination-confirmation process. Mishler and Sheehan (1993, 1994) claim a direct public opinion influence. Our results, with similar data but quite different model specification, leave us in the middle of this debate. We believe there is a trace of influence for both processes, but our results are too weak to leave us confident about the matter.

A Summary Analysis of Governmental Responsiveness

For a final summation of dynamic representation we slice across the institutional structure of American politics, returning to the familiar questions, Does public opinion influence public policy? and By what process? Our combining the policy output of the four institutions is, of course, a fiction: a single national public policy is not the average of independent branches. We "average" across different branches to provide a rough answer to a rough question. Here we

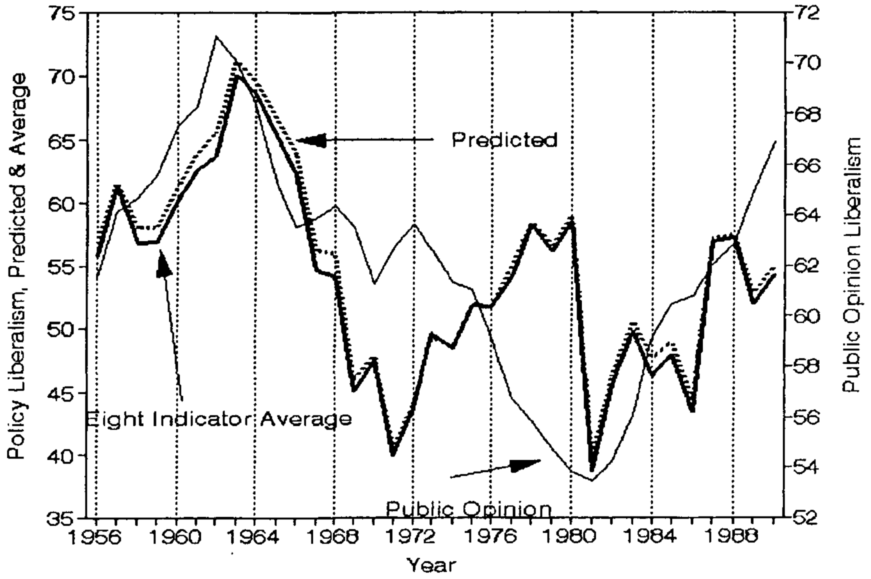
TABLE 5

Estimations of Global Representation

	MODEL AND ESTIMATOR			
VARIABLES	REDUCED FORM (KALMAN FILTER)	PARTY & IDEOLOGICAL COMPOSITION (REGRESSION)	FULL MODEL (KALMAN FILTER)	FULL MODEL WITH VIETNAM (KALMAN FILTER)
DYNAMICS (Y_{t-1})	.717 (.084)	—	.437 (.078)	.084 (.083)
PUBLIC OPINION$_{t-1}$	.360 (.101)	.542 (.235)	.502 (.130)	.712 (.096)
COMPOSITION	—	—	.414 (.121)	.704 (.091)
CUMULATIVE VIETNAM DEATHS	—	—	—	−.253 (.031)
MACROPARTISANSHIP	—	1.027 (.229)	—	—
Intercept	−5.806 (6.190)	−38.488 (23.925)	−24.696 (7.957)	−33.815 (6.591)
Number of cases	35	18	35	35
Adjusted R^2	—	.522	—	—
Measurement model (communalities)				
SENATE PERCENT LIBERAL WINS	.275	—	.471	.474
SENATE MEDIAN LIBERAL COALITION SIZE	.319	—	.447	.448
HOUSE PERCENT LIBERAL WINS	.391	—	.378	.393
HOUSE MEDIAN LIBERAL COALITION SIZE	.193	—	.192	.203
PRESIDENTIAL KEY VOTE LIBERALISM	.688	—	.668	.661
PRESIDENTIAL SUPPORT COALITION	.678	—	.647	.633
SUPREME COURT, CIVIL LIBERTIES	.636	—	.565	.563
SUPREME COURT, ECONOMICS	.482	—	.408	.397

FIGURE 5

Global Public Opinion and Global Public Policy: Predicted and Actual Policy



select two indicators from each of the four prior analyses (president, House, Senate, and Supreme Court) and then estimate representation as it works on the American national government as a whole.[29]

The estimates of Table 5 sum up the dynamic representation story. Column 1 produces a bare-bones model that posits policy as a simple and direct function of public opinion. While this representation misses much, it has the virtue of answering the first question without complication. And the answer is *yes*: public opinion influences public policy. We see that each point change in public opinion produces about a third of a point (.36) change in the overall policy of the federal government. The result passes statistical muster and produces clear evidence that public opinion matters.

However, the simple model misleads: it underestimates public opinion's power. The underspecification forces the public opinion measure to try to account for policy movement that can be more reasonably explained by our other factors and thus biases our estimates downward. Thus we turn to a richer model. As a proxy for composition, we average across institutions (PERCENTAGE DEMOCRATS in the two houses, PRESIDENCY dummy, and PERCENTAGE LIBERALS in the Court). This composite composition variable is clearly linked (col. 2) to both MACROPARTISANSHIP (1.03) and PUBLIC OPINION (.54)—no surprise.

We may now compare the turnover and rational anticipation linkages. We produce two estimates: one with and one without the Vietnam War variable. In the first (col. 3), we see that both composition and public opinion matter: the coefficients easily surpass statistical significance. Yet we can do better by taking account of the war: we show the estimates in column 4. As can now be seen clearly, both composition and opinion are unquestionably important for policymaking. Each percentage Democratic (or liberal) in composition produces seven-tenths (.704) of a point in policy. Equally, public opinion produces a rational anticipation response of seven-tenths (.712) of a point in policy. It is through elections *and* the anticipation of elections that public opinion drives governmental decisions.

Public opinion is substantially important. Combining the two paths, each point movement in preferences produces an electorally linked policy response (.382 = .542 * .704) and a direct rational anticipation response (.712), totaling 1.094 points in public policy. That is to say, there exists about a one-to-one translation of preferences into policy.

We get a better sense of the historical dynamic by examining Figure 5. Plotted here are measures of public opinion, public policy, and predicted policy. The first (in the light, solid line) is public opinion, with its liberal peaks during the early 1960s and late 1980s and its conservative peak around 1980. The dark, solid line represents policy, a simple average of our eight policy indicators. Without much work, it is clear that the two series are basically similar: policy reflects the timing and range of public opinion change.

Yet the two paths are not identical. Policy turned much more conservative during the late 1960s and early 1970s than the public demanded. Then, contrary to the continuing turn to the right, policy temporarily shifted leftward under Carter's leadership. Now look at the small dots that show predicted policy (the *state* variable taken from the last equation of Table 5). The exceptionally good fit is apparent. More important, the model is now able to account for the otherwise surprising conservatism just before 1972 and the liberalism of the late 1970s by including the Vietnam War and the composition variables. Thus, while the main part of policy moves in accord with public preferences, significant deviations can and do occur. Those deviations seem explicable but not by public preferences. Public opinion is powerful but not all-powerful.

Figure 5 takes us back to where we started, public policy preferences, and forward to the end of the story, the policy liberalism of American government, 1956–90. The point is that the two are a lot alike.

THE DYNAMICS OF INSTITUTIONAL RESPONSE

While we now know that national institutions reflect public opinion, we need to know more before we understand the character of dynamic representation. In particular, we want to know about both the speed of response and the nature of that response. These matters are, of course, central to understanding the dynamic character of democracy. In fact, much of the debate about the "checks and balances" built into the federal constitution centers around the ways that governmental response to "passions of the moment" might be tempered.[30]

There are two ways to think about institutional dynamics. First, and most straightforwardly, we want to understand the immediacy of coupling between public opinion and institutional policy. We want to know how complete the government's adjustment to public opinion is; that is, we want to know the extent to which this year's public policy reflects this year's opinion, as opposed to a residue of opinion impact over the past decade. A closely coupled system would have a relatively short institutional memory in the sense that policy will incorporate recent (as opposed to long prior) shifts in public opinion.

The second and perhaps more fundamental question is the extent to which policy reflects short-term changes in public opinion or whether it reflects the broader sweep of public opinion change. This question echoes the founding fathers' worries about passions of the moment, about whether a democratic system might veer wildly from one direction to the next in an attempt to respond to momentary (perhaps fickle) shifts in the public's views. Abstractly, we may divide the course of public opinion into (at least) two components. One represents a long-term change in the public's preferences, say, trends or cycles apparent over five- or ten-year periods. The second represents the movement around those longish trends, a demand that builds up over a period of months rather than years.

We may get some rough idea of the institutions' responsiveness by translating the dynamic terms (the coefficients associated with lagged policy output) into a time scalar, one that translates response into time units (here, years). We manipulate the *dynamic* ϕ coefficient to produce a *time constant* and a *settling point* for the system—numbers that give a sense of the speed at which the process reacts to change. The time constant represents the time over which about two-thirds of an exogenous shock's impact will be felt; the settling point represents the period over which 99% of the impact will be felt.[31] For the presidency (Table 1), we take the dynamic term (.192) and get a time constant of $1/(1 - .192)$, or 1.24 years. The settling point is then $3 * (1.24)$, or 3.72 years. In simpler words, PRESIDENTIAL LIBERALISM reacts mostly to the public opinion of the previous year and (almost) entirely to the public opinion of the past four years.

Similar calculations for the House and Senate produce time constants of 1.30 and 1.19 years respectively. While we cannot make distinctions between the three electoral institutions (these differences are not statistically discernible), one inference is clear: all are very quick. That is, to the extent that each institution represents public opinion, it represents (for the largest part) the previous year's public opinion. This may be compared to the Court's time constant of 2.31 years. The Court is identifiably slower than the electoral institutions (about twice as slow) as one might reasonably expect.

While this comparison carries no great surprise, it is reassuring to see that our intuitive understanding about the world bears up under systematic scrutiny. Electorally motivated politics are more immediately responsive to public opinion than are the politics of judicial decision. Note, however, that even the Court's response probably disappoints philosophical conservatives. By simple calculation, almost all of the Court's reflection of public opinion is driven by the events of the past seven years. While thus more tempered than "momentary passion," the Court does seem to float on the tide of political fashions, though of the decade rather than of the year.

Our second question asks whether it is the short-term or the long-term part of public opinion that drives policy. We may approximate an answer by turning to a model that bifurcates the relationship between opinion and policy into a long-term and a short-term response. We model policy (on the left-hand side) as a function of previous policy, previous public opinion, and current *changes* in public opinion (as well as our exogenous variables).[32] For our purposes, we need to examine the relative power of the "levels" of public opinion and the "changes" in public opinion. The first represents the longer-term relationship, the second, the shorter-term relationship. Table 6 produces the analysis for each institution.

The key test is the coefficient for current change in public opinion (row 3). For the Senate, the estimate is negative and statistically indiscernible from zero. Of course, even current levels of public opinion are relatively unimportant once we control for election outcomes. Again, the Senate eschews rational anticipation.

On the other hand, the presidency, the House of Representatives (especially), and even the Court

TABLE 6

Estimations of Long and Short-Term Components of Public Opinion (Kalman Filter Estimates)

	MODEL				
VARIABLES	PRESIDENT	HOUSE	SENATE	SUPREME COURT	ALL INSTITUTIONS
DYNAMICS (Y_{t-1})	.238	.064	.156	.562	−.089
	(.070)	(.127)	(.136)	(.160)	−.100)
PUBLIC OPINION$_{t-1}$ (level)	.752	1.290	.499	.228	.764
	(.252)	(.218)	(.403)	(.153)	(.101)
PUBLIC OPINION$_t$ (first difference)	1.332	2.757	−.801	.691	.325
	(.727)	(.761)	(1.727)	(.285)	(.344)
POLITICAL COMPOSITION	24.746	.847	1.119	.150	.866
	(2.312)	(.181)	(.384)	(.106)	(.116)
CUMULATIVE Vietnam casualties	—	−.508	—	—	−.266
	(.093)			(.033)	
Intercept	−25.259	−63.037	−43.543	−.354	−37.075
	(15.388)	(16.707)	(23.750)	(8.055)	(11.316)
Number of cases	35	35	35	38	35

evince sensitivity to short-term changes in public opinion. The coefficients for current changes in opinion (1.33, 2.76, and .69) pass a one-tailed test of statistical significance and thus suggest both causal and substantive implications.

First, the correspondence of short-term movement in both opinion and policy strengthens the causal inference. Were there only long-term association, our causal interpretation would be subject to the sorts of spuriousness that often characterizes covarying time series (classically, Granger and Newbold 1974). The fact that movement in the series corresponds even when statistically controlled for the long-term trends, reduces the chance that our inference is misleading.

On the substantive side, the three institutions seem to pay attention not only to the overall levels of public opinion (reflecting liberal and conservative eras) but also to the year-to-year shifts in opinion. This sensitivity is least, of course, for the Court. But for the presidency and the House, a point shift in current public opinion yields more than one point in policy. (This shift is, by construction, transitory. It disappears in the face of further "current" public opinion movement.) Hardly indifferent, these politicians are keen to pick up the faintest signals in their political environment.[33] Like antelope in an open field, they cock their ears and focus their full attention on the slightest sign of danger.

SOME REFLECTIONS ON AMERICAN POLITICS

The past four decades of United States history show that politicians translate changes in public opinion into policy change. Further, the evidence suggests that this translation varies by institution, both in the mechanisms that produce the link and in the nature of the dynamics.

Most important, dynamic representation finds strong support. Our work indicates that when the public asks for a more activist or a more conservative government, politicians oblige. The early peak of public opinion liberalism during the early 1960s produced liberal policy; the turn away from activism and the steady move toward conservatism was similarly reflected in national policy; and the recent 1980s upsurge of public demand for action was also effective (with the exception of the Court). To be sure, other things matter too. We have modeled a late 1960s shift rightward in policy (beyond that driven by public opinion) as a function of the Vietnam War's dominance over domestic political agendas. In addition, we modeled the shift leftward during the years of the Carter presidency (a shift contrary to the prevailing movement in public opinion) as a coincidence of compositional factors.

While we are confident that the basic result holds, we know that we do not yet fully understand movement in public policy. Nevertheless, the main story is that large-scale shifts in public opinion yield corresponding large-scale shifts in government action.

The link between opinion and policy is undoubtedly more complicated. While concentrating on policy response to opinion, we have seen little evidence of opinion reaction to policy. Elementary analyses generate contradictory inferences: the matter is subtle, the timing probably complex. We do know enough to assert that opinion reaction cannot explain the structural associations we uncover. We do not know enough to characterize the fuller relationship. This, of course, is a compelling subject for hard work.

Beyond the basic result, we can say that American national institutions vary in the mechanisms that produce responsiveness. It is the Senate, not the House of Representatives, that most clearly mimicks the eighteenth-century clockwork meant to produce electoral accountability. When comparing the effectiveness of turnover and rational anticipation, we find that for the Senate (and also for the presidency), the most important channel for governmental representation is electoral replacement. Equally responsive, however, is the House of Representatives. Its members employ rational anticipation to produce a similarly effective public policy response, without the overt evidence of personnel change. The Supreme Court appears to reflect public opinion far more than constitutionally expected; but, in comparison, it is the institution that responds least.

Finally, the dynamics prove interesting. Each of the electoral institutions translates immediately public opinion into public policy. That is to say, when electoral politicians sense a shift in public preferences, they act directly and effectively to shift the direction of public policy. We find no evidence of delay or hesitation. The Court, not surprisingly, moves at a more deliberate speed. But equally important, rational anticipation is based not only on the long-term trends in public opinion but also on year-to-year shifts. That is to say, politicians constantly and immediately process public opinion changes in order to stay ahead of the political curve. Understanding politics well, the constitutional framers were correct in expecting short-term politics to be a fundamental part of dynamic representation.

The United States government, as it has evolved over the years, produces a complex response to public demands. The original constitutional design mixed different political calculations into different institutions so that no personal ambition, no political faction, no single political interest, or no transient passion could dominate. We now see the founders' expectations about complexity manifest in contemporary policymaking. Constitutional mechanisms harness politicians' strategies to the public's demands. In the end, the government combines both short- and long-term considerations through both rational anticipation and compositional change to produce a strong and resilient link between public and policy.

APPENDIX A

Regression estimates in Table A-1 are respecifications of the Kalman filter analyses in the text. For each of the four branches, we employ the first indicator from the Kalman filter analyses. Because these indicators are metric-defining in the earlier analyses, the coefficients reported here are generally comparable. The *all institutions* regression uses an average of eight indicators, the two best (i.e., most valid) from each of the separate analyses. Comparability here is looser. The regressions are specified with a lagged dependent variable on the right-hand side to estimate dynamics and the same exogenous variables as in the Kalman filter analyses. One unavoidable noncomparability is that the regressions lose their first case to lagging.

TABLE A-1

Dynamic Representation Estimated by Regression

	MODEL				
VARIABLES	PRESIDENT	HOUSE	SENATE	SUPREME COURT	ALL INSTITUTIONS[a]
DYNAMICS (Y_{t-1})	.115	.142	.107	.523	.092
	(.106)	(.152)	(.167)	(.156)	(.132)
PUBLIC OPINION$_{t-1}$	.806	1.258	.352	.167	.566
	(.279)	(.380)	(.595)	(.312)	(.162)
POLITICAL COMPOSITION[b]	27.075	.147	1.464	.106	.695
	(3.550)	(.086)	(.479)	(.180)	(.160)
CUMULATIVE VIETNAM CASUALTIES	—	−.500			−.234
		(.121)			(0.050)
Intercept	−24.647	−53.354	−52.386	7.924	−25.667
	(16.679)	(31.035)	(32.940)	(16.158)	(9.751)
Number of cases	34	34	34	37	34
Adjusted R^2	.824	.636	.409	.329	.771

[a] The *all institution* measure is an equally weighted average of the two best (highest communality) indicators for each of the four institutions.

[b] Composition is an equally weighted average of % Democratic seats (House and Senate), the presidential party dummy variable, and the Supreme Court composition variable (% liberal).

TABLE A-2

Right-Hand-Side Variables for Tables 1–4

YEAR	PUBLIC OPINION[a]	PRESIDENT PARTY	% DEMOCRATIC, HOUSE	CUMULATIVE VIETNAM DEATHS THOUSANDS	% DEMOCRATIC, SENATE	COURT COMPOSITION[b]
1955	61.60	—	—	—	—	—
1956	64.16	0	53.10	.00	48	56.83
1957	64.73	0	53.33	.00	49	57.17
1958	65.63	0	53.33	.00	49	57.02
1959	67.56	0	65.06	.00	64	57.02
1960	68.31	0	65.06	.00	64	57.02
1961	71.04	1	60.46	.00	64	57.02
1962	70.02	1	60.46	.00	64	57.02
1963	68.36	1	59.08	.00	67	61.25
1964	65.35	1	59.08	.00	67	61.25
1965	63.49	1	67.82	1.35	68	61.25
1966	63.89	1	67.82	6.30	68	60.17
1967	64.32	1	56.78	15.55	64	60.17
1968	63.55	1	56.78	29.95	64	61.81
1969	61.24	0	55.86	39.27	58	61.81
1970	62.67	0	55.86	43.49	58	56.94
1971	63.63	0	58.62	44.87	55	56.94
1972	62.60	0	58.62	45.17	55	47.67
1973	61.29	0	57.01	.00	57	47.67
1974	61.02	0	57.01	.00	57	47.67
1975	59.13	0	66.67	.00	61	47.67
1976	56.75	0	66.67	.00	61	45.60
1977	55.85	1	65.52	.00	62	45.60
1978	54.75	1	65.52	.00	62	45.60
1979	53.85	1	62.76	.00	59	45.60
1980	53.44	1	62.76	.00	59	45.60
1981	54.22	0	55.40	.00	47	45.60
1982	56.09	0	55.40	.00	47	41.18
1983	59.15	0	61.15	.00	46	41.18
1984	60.51	0	61.15	.00	46	41.18
1985	60.79	0	58.16	.00	47	41.18
1986	62.06	0	58.16	.00	47	39.50
1987	62.87	0	58.62	.00	55	39.50
1988	64.93	0	58.62	.00	55	39.97
1989	66.91	0	59.31	.00	55	39.97
1990	67.20	0	59.31	.00	55	39.97

[a] t − 1 value used.

[b] Justice lifetime liberalism.

APPENDIX B

These two tables document first the right hand side variables as used in Kalman filter analyses of Tables 1–4, and then the "state variables" estimated from full model specification in those tables. The state variables are empirical estimates of the latent policy construct, weighted linear combinations of the right hand side variables. Not included, for space limitations, are the measured indicators of policy or the components of the composition regressions. These are available from the authors on request.

Notes

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1. For an instance of modeling representation of public opinion from a theoretical perspective similar to what we advance here, see Bartels 1991.

TABLE A-3

Estimated Latent Policy Liberalism Variables for Tables 1–4

YEAR	PRESIDENT	HOUSE	SENATE	SUPREME COURT
1956	42.31	72.18	54.89	56.56
1957	57.10	69.47	66.82	56.83
1958	39.96	73.23	56.34	58.15
1959	34.84	85.72	67.46	52.34
1960	41.18	85.60	63.52	54.52
1961	68.88	77.34	75.64	48.97
1962	65.74	81.04	60.67	56.70
1963	65.40	73.17	84.02	71.83
1964	66.75	75.63	79.78	66.46
1965	59.70	82.70	79.92	55.90
1966	60.48	73.29	64.47	56.39
1967	54.23	54.61	56.01	55.45
1968	54.94	61.88	42.51	52.30
1969	31.26	48.39	55.68	44.22
1970	28.18	50.69	54.47	46.05
1971	24.20	43.34	48.45	39.52
1972	25.94	44.77	58.01	38.83
1973	25.18	61.58	72.21	41.70
1974	27.41	63.08	68.22	41.03
1975	29.50	73.47	76.02	43.02
1976	21.30	73.79	71.75	44.16
1977	57.32	67.61	54.67	43.09
1978	51.60	55.70	83.84	47.85
1917	55.60	62.92	70.58	43.81
1980	52.69	65.95	74.24	47.62
1981	23.70	47.05	15.99	48.18
1982	23.63	62.52	27.97	49.97
1983	25.38	75.74	43.16	48.10
1984	27.06	75.29	23.80	46.58
1985	24.20	74.04	35.51	47.38
1986	21.58	79.16	28.35	40.22
1987	23.69	85.41	80.75	49.22
1988	26.78	85.29	75.42	49.51
1989	27.34	82.36	59.02	44.30
1990	26.92	84.47	69.04	45.40

2. To be sure, the idea that policy reproduces itself is not merely a naïve null model. Policy creates its own constituencies—policy networks—that have incentives to perpetuate themselves. This *positive feedback* is the standard inside-the-Beltway expectation.

3. Note that this doubly dynamic model differs at its root from the typical cross-sectional view of representation. The cross-sectional (or geographic) model pits permanent interests against one another with change owing itself to political strategy and fluctuations in political power. The dynamic representation model incorporates systemic learning, or collective deliberation, through which the government and the social system consider and then reconsider the merits of policy decisions.

4. This verbal sketch represents what is surely a more complex process at equilibrium. Some politicians will, of course, make mistakes or take principled policy stands. Folklore, however, suggests that most share in their appreciation of the electoral imperative and act upon it. Our assumption about the independence of electoral surprises, policy preferences, and politicians' professional sophistication is, to be sure, a simplification.

5. This focus on anticipation as the quintessential behavior of elected politicians has much in common with Arnold's (1990) statement about the congressional side of government. Note that given the enormous personal commitment required for launching a political career, politicians must plan on winning a *sequence* of elections before the expected rewards exceed the costs (Erikson 1976). Thus they must make judgements about a series of future moments, not merely the next press conference. A House member or a Senator must anticipate the electoral climate over a two- to six-year time horizon. Clearly, savvy politicians will pay more attention to climate change than to the swirl of day-to-day weather patterns. They must look for trends, not momentary movements.

6. Their careers at stake, politicians energetically consult multiple sources of information about the public's policy judgements—opinion polls, constituency newspapers, chats with ordinary voters, monitoring mail, "lobbying" efforts, and comparisons with other Washington politicians. (Note that while we, as scholars, rely solely on public opinion polls to do our work, politicians surely do not.)

Not all the change derives from expediency. Politicians also have conversations with close friends and supporters, their *personal* constituencies (Fenno 1978), whose views may genuinely alter the politicians'. Further, politicians may be influenced, again genuinely, by changes in the "intellectual" climate that also simultaneously changes public judgements. As Kingdon (1973) suggested some time ago, personal views and strategic positions may become indistinct in some politicians' minds.

7. We here ignore the details of policymaking. Clearly, the major institutions of the U.S. government differ considerably in their internal structure and thus will differently reflect changes in public preferences. Theory and empirical evidence are not absolutely clear about how these structural differences will matter.

However, for intriguing evidence on the matter, see Huber and Powell (1994). Their examination of 38 governments in 11 nations (not including the U.S.) indicates that coalition governments, better than clear-cut majoritarian governments, represent median voter preferences. Thus, we should not be surprised to see that the loose coalition bargaining that typifies U.S. politics might faithfully represent public opinion.

8. Here we add to previous evidence on this matter. Compared to other work, we focus on policy more broadly defined and over a longer time horizon. For careful empirical analyses of these issues, see Asher and Weisberg 1978; Brady and Sinclair 1984; Bartels 1991; and Page and Shapiro 1983, 1992.

9. This evidence should not be taken as disconfirmation of the reaction thesis. The coefficient does turn out to be negative (in the Granger setup) as expected. And the annual aggregation used here may undermine the case for opinion reaction both by making significance difficult to achieve (from small N) and by misgauging the time sequence of response. In some exploratory analyses we have used current opinion to predict policy-making with the common result of slightly attenuated coefficients. That small attenuation may well be due to opinion response. Such effects can be seen, for example, within 1981, where early sharp conservative moves in policy direction produced movements away from conservative preferences within the calendar year.

10. Attacking the problem one indicator at a time is even more problematic. We believe that the indicators contain both specific and common (i.e. valid) variance. When they appear alone on the left-hand side, then not "policymaking" but "policymaking plus something specific to the indicator" is being modeled. That produces divergent results, depending upon choice of indicator, when we believe there is but one underlying concept. This may be seen as a seemingly unrelated regressions issue; modeling one indicator at a time makes inefficient use of the information we possess about indicator mutual associations.

11. See Kellstedt, McAvoy, and Stimson (n.d.) and Beck (1990) for a treatment of the Kalman filter as applied to political science research. Watson and Engle (1983) develop

both the logic and estimation technology of the Kalman filter in earlier papers.

12. Because the latent state variable has no identified metric, by convention it is assigned one from one of the indicators. Here the first indicator measurement model is set up with a fixed slope of 1.0 and a 0.0 intercept, so that the state variable takes on the metric of this indicator.

13. See also Durr (1993) and Mishler and Sheehan (1993) for applications of the mood concept and measure. Here we employ an updated version of the measure (see Appendix B).

14. Although derived from all domestic policy preferences, the major dimension of mood is unrelated to the abortion controversy.

15. The essential problem is year-to-year comparability of roll calls or lack thereof. If, say, an ADA scale aggregated for all members increases from one year to the next, we cannot know for certain whether the second Congress was more liberal or the ADA picked an easier set of votes to scale in the second year. Having multiple indicators is thus important because different "rating houses" are unlikely to have identical motives in shifting the liberal-conservative anchor points for their scales. Of course, it is important that the different rating houses are interested in a similar common dimension. For reassuring evidence on the matter, see Smith, Herrera, and Herrera 1990.

16. The logic here is much the same as the commonly used "conservative coalition" criterion. We prefer it because we are uncomfortable with the assumption that the position of the American South in party and ideological politics is constant over time.

17. This speaks to concerns such as Riker's (1962) size principle, which would suggest that gains on one or the other side would not result in larger coalitions but, rather, ratchet up the content of votes so that "better" votes could be won by minimum winning coalitions. Recent research (Mouw and MacKuen 1992) shows that the party leaderships and presidents do, in fact, alter their political agendas to accord with political circumstances and with the strength of their coalitions. Thus, from a theoretical point of view, it is critical that we include *both* wins and coalition size. (Empirically, as we shall see, this subtlety matters little for our aggregate analyses.)

18. The subset of domestic policy legislation is employed. Of those bills, a small number is screened out because the outcomes have a voting split (more than 80% in the majority) suggesting consensus rather than ideological disagreement. The final set of key votes (N = 668 for the years 1956–90) is all domestic issues that can be coded as to direction with outcomes in the 20–80 range.

19. We are grateful to Jeff Segal for aiding our task by providing his solicitor general data. We have supplemented the Segal data to fill out the time period under analysis.

20. Votes that are unanimous or with a computed difference in aggregate liberalism between majority and dissenting positions of less than 10 points are classified as nonideological. Our two-step approach is motivated by concern that human coding of case content is likely to err from time to time: things are not always what they seem to be. Such errors readily succumb to aggregation over the full career of the justice, producing a judgement-free (if arbitrary) classification in the second step.

21. CIVIL RIGHTS AND LIBERTIES includes the data base categories "Civil Rights," "First Amendment," "Due Process," and "Privacy;" CRIMINAL PROCEDURE is the "Criminal Procedure" category alone; ECONOMICS includes "Unions" and "Economics;" OTHER is everything else.

22. We drop the OTHER category because it fits the overall pattern so poorly. This category is less distinctive over time than the others, as would be expected from its catch-all character. More than the others, it probably includes many classification errors—cases where liberals vote against conservatives without any ideological case content on which to disagree.

23. We make no special provision for transitions.

24. We make no claim for this particular model. It is, after all based on only nine observations. ML probit or logit models fail for lack of real variation.

25. The communalities are estimated from the (squared) empirical correlations of the indicators and the state variable. They can be interpreted, in a standard "factor-analytic" way, as keys to the substantive meaning of the common dimension. However, we refrain from leaning on a validity interpretation because in these models the communalities seem to be dominated, instead, by the relative measurement reliabilities of the indicators.

26. Note that the model includes a marker for the Vietnam War. A model without such a specification leaves public opinion nonsignificant. More distressful, it also tells us that the party composition of the House has no effect on policy outcomes. These results demand improved specification. Close inspection of the time path of both opinion and policy, however, convinces us that policy became unexpectedly conservative during the late 1960s and early 1970s—just when controversy over the war took over American politics. Our contextual understanding of the politics of the era indicated that this Vietnam (cumulative American casualties) proxy properly accounts for an exogenous (i.e., not opinion-related) deflection from a theoretical path.

27. This is not to say that such a linkage does not exist, only that we cannot identify it. A pooled analysis (Stimson 1991) finds statistically significant electoral linkages between MOOD and House elections. The result shows some sensitivity to sample period as well. The sample we use for the election analyses is the set of elections of 1954–88, that produced the members whose behavior we model for 1956–90.

28. The metrics are comparable: each composition variable is scaled in percentages of the respective house. This point is further underscored when we note that the public preference-composition model accounts for about 54% of the House variance and about 74% of the Senate variance.

29. We have forced the "variance" of each of the measures to be equal and fixed the scalar translations of the indicators to unity. This trick corrects for a tendency for the estimation to track the Senate (the most opinion-sensitive institution) and ignore the rest.

30. This recalls a famous interchange between George Washington and Thomas Jefferson. When Jefferson asked over the breakfast table why a second legislative chamber had been created, Washington asked, "Why did you pour coffee into your saucer?" "To cool it," Jefferson answered. "Even so," said Washington, "we pour legislation into the senatorial saucer to cool it" (quoted in Sundquist 1992, 27).

31. The number is simply $1/(1 - \phi)$, where ϕ is the dynamic term. The settling point is simply three time constants (see MacKuen 1983). We model a single dynamic for all inputs.

32. This format is similar, but not identical, to the popular *error correction mechanism* (Davidson et al., 1978) or cointegration (Engle and Granger 1987) model. The qualitative inferences, about short-run and long-term change, will be similar. For this timing analysis alone we drop our usual concern for the time ordering of opinion and policy, using opinion at time t. This may have the effect of slightly attenuating the coefficient for current opinion, combining the strong and positive representation effect with the weak and negative policy reaction effect.

33. We strongly suspect that this sensitivity makes political sense. The efficiency of House short-termism passes a statistical test. We regressed current policy on previous policy and previous opinion and current change in opinion (and composition and the rest of the standard specification), adding *public opinion two years' hence* (and hence beyond the next House election). Intriguingly, future public opinion exerts an independent statistical influence on current policy change. In other words, House policy anticipates true changes in public opinion. Further, the previously robust effect of public opinion change now disappears. Presumably, politicians use information about public opinion change to make inferences about likely future directions. While all this satisfies our theoretical perspective, we are hesitant about making strong

claims. The data are far too sparse to produce firm conclusions of this subtlety.

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CAMPAIGN CONTRIBUTIONS AND ACCESS

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An important and pervasive view of campaign contributions is that they are given to promote access to successful candidates under circumstances when such access would not ordinarily be given. In this story, access is valuable as it offers groups the opportunity to influence legislative decisions through the provision of policy-relevant information. Under complete information regarding donors' policy preferences, I argue that this model predicts a negative relationship between contributions and the extent to which the groups' and the recipient legislators' preferences are similar. However, one of the more robust empirical findings in the literature is that this relationship is positive. Relaxing the informational assumption on donors' preferences, I reexamine the access story with a model in which campaign contributions can act as signals of policy preference and the (informational) value of access to any agent is endogenous.

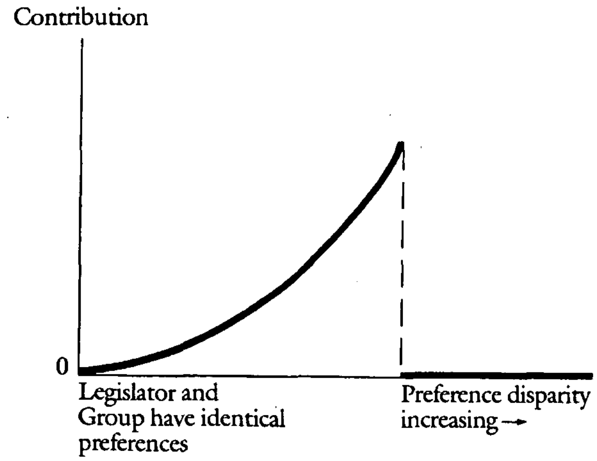
The relationship between campaign contributions and *access* to legislators is typically seen as a close one, although exactly what it is that makes access worth "buying" is subject to some debate (Hall and Wayman 1990, 800). There are (at least) three, by no means mutually exclusive, perspectives on what access secures. From the first perspective, the one most prevalent in the formal literature, access is simply a euphemism for a donor securing the given private return on an investment of a contribution; indeed, access is typically implicit (Baron 1989, 1994; McCarty and Rothenberg 1993; Snyder 1990). A second perspective is that access is primarily symbolic, groups seeking access to signal their importance and so maintain and increase their membership. Under this view, access is granted by a legislator purely to secure contributions, and any consequent group influence over legislators is largely incidental ("Groups that pressure or antagonize policymakers may forfeit access and thus lose the symbolic benefits of being consulted and the opportunities for credit-claiming that go with it" [Hayes 1981, 86]). Finally from the third perspective, access is essential and has little to do with any more or less implicit quid pro quo model of contributor decisions or with symbolic concerns, the focus being, instead, on information (Bauer, Pool and Dexter 1963; Hansen 1992; Milbrath 1960; Rothenberg 1992; Wright 1990). And this is the dominant perspective within the descriptive literature: "PAC officials are adamant that all they get for their investment is access to congressmen—a chance to 'tell their story'. Political analysts have long agreed that access is the principal goal of most interest groups, and lobbyists have always recognized that access is the key to persuasion" (Sabato 1985, 127).[1] I am here concerned with the third perspective on access.

The *canonic story* connecting campaign contributions to access is predicated on the premise that both time and information are valuable. When an issue arises about which a legislator is uncertain, he or she will seek information from more or less interested parties; and given that the legislator cannot see everyone with something to say about any given issue, he or she will choose whom to listen to at least in part on the basis of who provided campaign contributions (Herndon 1982; Langbein 1986; Shlozman and Tierney 1986, 246). However, although intuitively plausible, there are some difficulties with the story.

Given that a legislator is uncertain about what position to adopt on some issue, the legislator will, other things being equal, seek information. Since legislators' actions depend in part on their information regarding the consequences of actions, information provision by interested groups is inherently strategic. And if information is costly to verify, it is known that the amount of information an informed agent (lobbyist) can credibly transmit to an uninformed decision maker (legislator) is increasing in the extent to which these individuals' preferences over final consequences are coincident.[2] That is, the more like the legislator the lobbyist is, the more valuable that lobbyist will be to the legislator on informational grounds and the more influence that lobbyist that can be expected to exert on the legislator's decisions. Consequently, if legislators know the preferences of the prospective lobbyists, they will, ceteris paribus, choose to listen to those lobbyists whose underlying preferences most closely reflect their own; and this will be true *irrespective* of any campaign contributions. Moreover, if a group knows that its preferences are far enough away from those of the legislator that even granted access, it cannot be influential, then that group has no incentive to devote costly resources to securing access. In sum, if the rationale for access is informational, access will only be granted to groups whose preferences over consequences are sufficiently close to those of the legislator to permit credible information transmission and who may thus be influential (i.e., offer information that can affect the legislator's decisions). Only those groups who fall within this category will be willing to pay for access; but given information is valuable, the legislator will be willing to grant access to such groups independent of any financial incentive.

FIGURE 1

Prediction of the Canonic Model



For money to alter the preceding conclusion there must at least be some trade-off for the legislator between informational gains and dollars. But then, up to the point at which preferences are sufficiently disparate to preclude any credible information transmission, there should exist a positive correlation between campaign contributions and the degree to which contributors' and legislators' preferences diverge, and beyond this point contributions (for access) should be zero (see Figure 1).[3] However, to the extent that legislators' actions are indicators of their legislative ideologies, or preferences, empirical work has consistently estimated the relationship between contribution (conditional on this being positive) and preference disparity to be negative. Poole and McCarty (1993) test directly the proposition that a political action committee's contributions are positively related to the extent to which its ideology matches that of the legislator (see also, e.g., Kau, Keenan, and Rubin 1982; Poole and Romer 1985; Saltzman 1987; Welch 1980).[4] Moreover, according to at least one estimate, only one in three of those interest groups with lobbying organizations in Washington has a political action committee (Gais 1983), suggesting that contributions are by no means necessary for securing access.

Insofar as the value of access to interest groups and legislators is informational—the value for interest groups deriving from the possibility of influencing legislative decisions through the strategic provision of information and that for legislators deriving from the possibility of making more informed decisions—the preceding discussion indicates a conflict between the canonic theory and the empirical pattern of contributions. There are basically two approaches to dealing with the conflict, short of rejecting the access model of campaign contributions entirely. The first is to argue that in fact purely access-oriented contributions *are* negatively correlated with preference similarity but that other, positively related, motivations for contributing dominate this relationship statistically. But while such a claim might indeed be correct, the operational difficulties of disentangling the separate effects empirically seem severe, if not prohibitive. The second approach is to build on the observation that, ceteris paribus, the expected value of access is increasing with the extent to which the legislator's and the interest group's preferences are similar. In particular, suppose there is some uncertainty on the part of the legislator regarding the underlying preferences of the donor. Under such uncertainty campaign contributions can fulfill a role other than simply purchasing legislators' time; specifically, they can signal the extent to which any informational lobbying by the group would be valuable to the legislator.[5] Prima facie, the signaling role of money can be expected to yield a negative empirical relationship between preference disparity and contribution size because, as discussed, the value of access is increasing in the extent to which groups' and legislators' preferences are similar. However, the value of access depends at least in part on legislators' beliefs about this preference similarity, which in turn depend on the contributions offered. In other words, given that access is sought purely to provide information and given that such information provision is strategic, the value of access to a group and the willingness of the group to contribute campaign contributions to secure access are jointly determined.

I shall consider the signaling role for campaign contributions more formally with a stylized model in which a group, with preferences known surely only by the group itself, chooses a campaign contribution to give to an incumbent legislator prior to becoming informed about an issue of concern that may or may not arise in the legislature. If the issue arises then the group alone becomes informed and the legislator chooses whether or not to grant (costly) access to the group. If access is denied, the legislator simply acts on his or her prior beliefs about what best to do with the respect to the issue, but if access is granted, the group advises the legislator about the action to take and the legislator uses this advice to update his or her beliefs and arrive at a decision. The focus of the model is then the relationship among campaign contributions, preferences, and the value of access.

The key premise of the model is that the legislator has some uncertainty about the policy preferences of the group. To the extent that this is not a reasonable approximation to the truth, the model, along with the motivating discussion, suggests that the informational perspective connecting contributions to access is suspect. However, there are circumstances under which the premise is plausible. For example, internists and specialists within the American Medical Association (AMA) frequently hold distinct views on particular health policy issues, and exactly what the "AMA position" is on such issues will reflect compromise and bargains internal to the organization as a whole; it is then reasonable to assume that these compromises and bargains are not common knowledge and therefore that the "AMA position" is subject to uncertainty. This kind of uncertainty is reflected in the surprise of legislators at the National Rifle Association's (NRA's) neutral position on the Bork nomination (McGuigan and Weyrich 1990, 78–80). Although the NRA's general policy preferences are no secret, their stance on this particular issue was clearly subject to uncertainty for the politicians. Similarly, there is evidence of groups lobbying on issues outside their imputed interests, which almost surely induces some uncertainty about the groups' preferences over such issues (Browne 1990, 1991). Finally, and perhaps most important, is that the sheer numbers of groups involved in lobbying prohibits legislators and the staffs from possessing complete knowledge of groups' preferences. In discussing his survey results on lobbying over agricultural policy, Browne remarks, "The most striking point that these officials raised about the expanded universe of agricultural interests was about the confusion it creates; 'Who are these guys?' was the often-repeated question" (1991, 359).

Given that there is some uncertainty on the part of legislators regarding donors' basic preferences, it turns out that the expectation regarding campaign contributions and preference similarity can be fulfilled in the model subject, however, to two qualifications. First, campaign contributions are incapable of fully revealing group preferences; so when access is granted, the legislator remains under some uncertainty as to the exact preferences of the lobbyist. Second, if the equilibrium contribution schedule reveals any information at all about the donor's preferences, it cannot be monotonic. In particular, the schedule can be U-shaped, with relative extremists making the same contribution as groups whose preferences closely reflect those of the legislator; but when this occurs, the probability mass is typically on preferences "close" to the legislator, so the statistically observed relationship will be positive.[6] Each qualification has substantive implications for the access model of contributions, and these are considered further in the concluding discussion.

I shall describe the model and develop an example to illustrate the intuition behind the principal results, then offer some caveats and implications. Formal arguments supporting all of the results are confined to an appendix.

MODEL

Agents and Preferences

There are two agents in the model: a group and a legislator. Both the group and legislator have preferences over the consequences of a particular policy issue that may or may not arise in the coming legislative session. (For example, the issue may be health insurance, and both the AMA and legislators have preferences over the final cost of health care induced by any legislation on rates.) Assume that the issue and its consequences are one-dimensional objects and assume further that at least ex ante, the consequences of any legislation are known only with uncertainty. The basic setup closely follows Gilligan and Krehbiel 1987. Specifically, let $t \in [0, 1]$ be the realization of some random variable and let $a \in \mathbb{R}$ denote the legislative action taken (if necessary) on the issue. Let $g(t|\cdot)$ be a probability density function (pdf) characterizing the legislator's beliefs over t conditional on any information he or she might have. Then the legislator's ex ante evaluation of the action a is given by,

$$E[u(a, t)|\cdot] = -\int_0^1 (t-a)^2 g(t|\cdot)\, dt. \qquad \textbf{(1)}$$

The legislator who knew the value of t for sure, therefore, would choose the action $a = t \in \mathbb{R}$, since this maximizes the payoff. This payoff function can reflect either the legislator's policy preferences or be induced from more primitive electoral concerns in which the likelihood of reelection depends at least in part on the policy decisions taken by the legislator.

Similarly, the group's ex ante evaluation of action a is given by

$$E[v(a, t; x)|\cdot] = -\int_0^1 (x+t-a)^2 h(t|\cdot)\, dt, \qquad \textbf{(2)}$$

where $h(t|\cdot)$ is the pdf describing the group's conditional beliefs regarding t, and $x \in X \subseteq (-\infty, \infty)$ is the group's most preferred consequence from any legislative action. Assume that X is an interval. For any value of t, the group's most preferred legislative action is thus $x + t$. It follows that the parameter x measures the extent to which the group and the legislator have diverse preferences: for any given value of t, the group's most preferred action is strictly increasing in x. So when x is positive, the group prefers higher actions than those most preferred by the legislator—and the converse when x is negative, with the difference between the two most preferred actions being given by the absolute value of x.

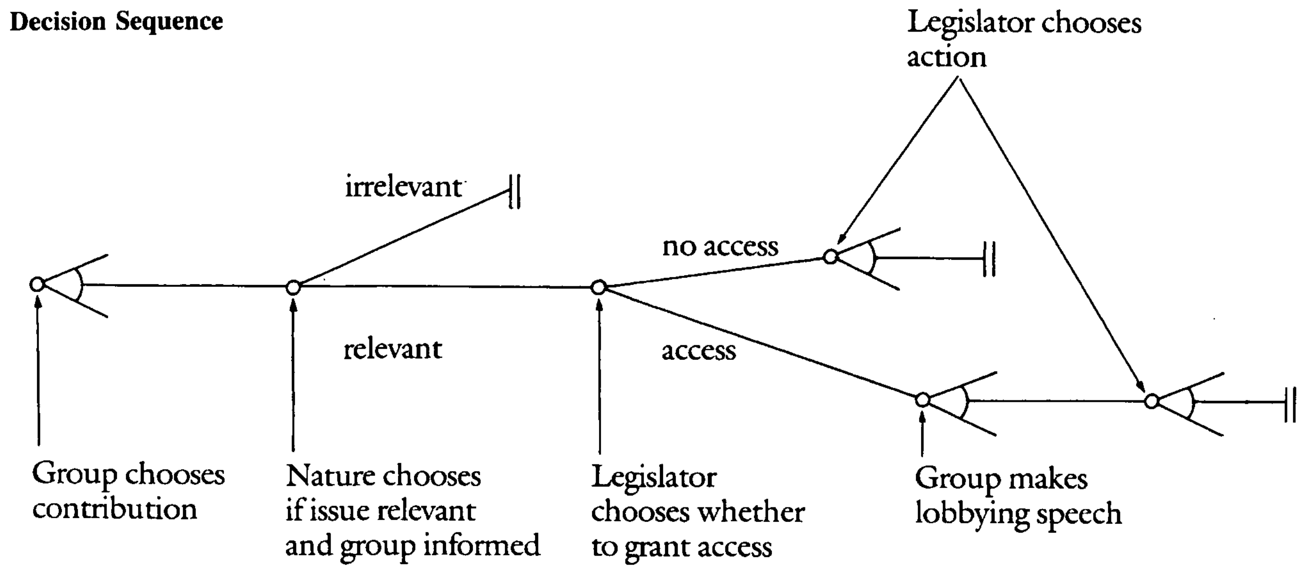
The payoffs to both legislator and group if the issue does not arise during the legislative session are strategically irrelevant; so without loss of generality, they are set equal to zero. Further, conditional on the issue arising in the legislature, assume that the status quo policy is strategically irrelevant. In other words, there are no possible circumstances (as delimited by $t \in [0, 1]$) under which the group or the legislator prefer to remain with the status quo. Although restrictive, the qualitative results connecting campaign contributions to group and legislator preferences seem unlikely to depend on the assumption. Relaxing it, however, greatly complicates details of the lobbying stage of the game.[7]

Decisions

The sequence of decisions is given in Figure 2. The group moves first by giving a campaign contribution $c \in \mathbb{R}_+$ to the incumbent. The canonic access motiva-

FIGURE 2

Decision Sequence


Legislator chooses action
irrelevant
no access
relevant
access
Group chooses contribution
Nature chooses if issue relevant and group informed
Legislator chooses whether to grant access
Group makes lobbying speech

tion for contributing being studied here is essentially concerned with gaining legislative access for purposes of advocacy. Consequently, I abstract from details of the electoral process.

There are two (not mutually exclusive) ways to interpret the reduced form of the electoral process implicit here. The first is to suppose that the election takes place immediately following the group's contribution. Since any group's contribution is small relative to total contributions, assume that the group treats the probability that the candidate will win as exogenous; assume further that (at least in equilibrium) the group gives a contribution to at most one candidate in any election. Then the candidate either wins or loses: if the candidate wins, then the game proceeds exactly as will be described; if the candidate loses, then, for our purposes, the game is over. So given this story for the electoral stage, the model described here begins after the group has made its categorical decision on which candidate to support, adding the probability of electoral success into the model adds nothing but notation.

The second way one can think of the model with respect to the electoral process is to assume the legislator is an incumbent interested, inter alia, in reelection. From this perspective, the contribution is given early in the legislative session for the next election, and the model applies without qualification. In any event, in what follows I suppress further consideration of the electoral stage and, consistent with the canonic access story, model the value of contributions to the legislator in terms of the opportunity cost of giving time to listen to the group rather than do something else.

Once the contribution is given and the legislator is in office, Nature decides with probability $q \in (0, 1]$ whether the issue requiring legislative action arises. The idea here is that money is given before details of the legislative agenda are revealed. For example, while it may be common knowledge that the health care industry is to come under scrutiny during the next legislative session, it is less clear exactly which aspects of the industry—or what sort of policy issues falling under the health care rubric—will receive specific attention. Thus potentially interested groups give money in an effort to secure access to key legislators should the need arise (Sabato 1985).

If the issue does not arise (which occurs with probability $1 - q$), then the game is over; but if it does arise (with probability q), Nature reveals the true value of the unknown parameter t privately to the group.[8] (To support any access role for contributions in the model, it is important that the group not be—or, at least, not be known to be—informed at the time it chooses its campaign contribution. See the concluding discussion.) Conditional on the issue arising and the group becoming informed, the legislator decides whether or not to grant access to the group. The legislator who does not grant access simply chooses an action on the basis of prior beliefs concerning the value of t, and the game ends with all players receiving their respective payoffs. On the other hand, the legislator who does grant access is lobbied by the group, which makes a speech about its information on t, following which the legislator takes an action, the game ends, and payoffs are distributed. The group's lobbying speech is costless; in particular, it is free to say anything it wishes about the value of t or anything else, and lying is no more difficult for the group than is telling the truth (Crawford and Sobel 1982).

Assume that granting access is costly to the legislator; he or she has a positive opportunity cost of time. Consequently, access will only be granted if the expected value of any subsequent information transmission at the resulting lobbying stage exceeds the cost of granting the time to listen to the group. Because campaign money is itself valuable and is in part a substitute resource for time, the legislator's opportunity cost of granting access may be decreasing in campaign contributions. In sum, let $k(c)$ denote the cost of granting access conditional on receiving a contribution $c \geq 0$, and assume that $k(0) > 0$ and $k'(c) \leq 0\ \forall c \geq 0$.

Information

The legislator's preferences are common knowledge. The group's preferences are common knowledge up to the value of its ideal point, x, which is private information to the group. The legislator has common knowledge prior beliefs about x described by a differentiable and strictly increasing distribution function $F(x)$ on X. Both the group and the legislator know the probability, q, that the issue of concern will become legislatively relevant. If it does become so relevant, then Nature reveals the true value of t privately to the group, which is therefore asymmetrically informed relative to the legislator; both agents share a common prior on t, $g(t)$, given by the uniform distribution on $[0, 1]$; thus $g(t) = 1$ for all $t \in [0, 1]$.

Strategies and Equilibrium

The group's *contribution strategy* is a map $\gamma: X \to \mathbb{R}_+$. Thus for any group with ideal point $x \in X$, $\gamma(x) \geq 0$ is the contribution donated by the group to the legislator. The restriction to pure strategies here is without loss of generality. Nature then, with probability q, chooses whether the issue of concern becomes legislatively relevant and the group becomes informed. Conditional on the issue arising, the legislator decides whether to grant access to the group. The legislator's *access strategy* is a map $\alpha: \mathbb{R}_+ \to [0, 1]$, where $\alpha(c)$ is the probability of the legislator granting access to a group that has contributed c to the campaign fund.

If access is denied, the legislator simply makes a decision about what action to take; but if access is granted, the group and the legislator play the Crawford and Sobel (1982) Sender/Receiver game. Specifically, *given* that access is granted (so the issue is relevant and the group is informed), the group sends a cheap talk message about its information according to a *lobbying strategy* $\lambda: X \times [0, 1] \to M$, where M is some arbitrary uncountable message space. Thus $\lambda(x, t)$ is the speech made by an informed group of type x that has observed the true value t. To save on notation, write $\lambda \equiv \varnothing \in M$ to describe the message "heard" by the legislator if the latter denies access and there is no lobbying by the group. Then the legislator's *decision strategy* is a map $\delta: \mathbb{R}_+ \times M \to \mathbb{R}$. Recall that by convention $\lambda(\cdot) \equiv \varnothing$ if the group is denied access; therefore the dependency of δ on the legislator's access strategy is implicit in M. Because the legislator's preferences over consequences are quadratic, the restriction to pure decision strategies is without loss of generality.

The equilibrium concept used is *sequential equilibrium*—loosely, a list of strategies $(\gamma^*, \alpha^*, \lambda^*, \delta^*)$ and a set of beliefs is an equilibrium if (1) at every decision stage each agent takes an expected-utility-maximizing action conditional on the other's behavior and on the agent's beliefs at that stage and (2) beliefs are derived from Bayes' rule where defined. A formal definition is given in the Appendix.

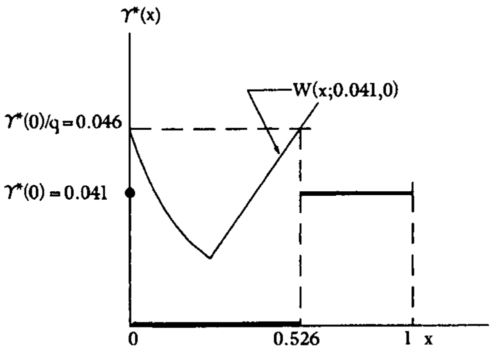
If access is granted, I shall refer to the lobbying and decision strategies jointly as the *lobbying subgame* (λ, δ). It is well known that in the lobbying subgame there exists an equilibrium in which no information on t is transmitted and the legislator simply chooses an action on the basis of prior beliefs (Crawford and Sobel 1982). In such circumstances, no type of group would seek access. Consequently, I shall focus on *influential* equilibria (i.e., equilibria in which the legislator's decision strategy is not constant in the message received). Thus in any influential equilibrium to the lobbying subgame, the group has at least two speeches it can make, each one of which induces the legislator to take a distinct action. If a speech does induce the legislator to take a particular action, say that the speech *elicits* that action. Thus an influential equilibrium is one in which at least two actions are elicited by possible speeches made under the lobbying strategy, λ. And say that an equilibrium lobbying strategy λ is *most influential* if there is no other equilibrium lobbying strategy that elicits more actions from the legislator. Unless stated explicitly otherwise, a maintained assumption throughout the analysis is that conditional on access being granted, the most influential equilibrium lobbying strategies are always used. The main justification for the assumption here is that it is conservative: most influential equilibria are ex ante most preferred by all agents and therefore offer the best opportunity for access to be valuable.

A NUMERICAL EXAMPLE

Suppose, on the issue of concern, that the group is known to prefer relatively higher actions than the legislator under any circumstances. In terms of the model, this amounts to $X = [0, \bar{x})$ for some $\bar{x} > 0$. Then the principal result is that if campaign contributions provide any further information to the legislator about the contributor's preferences over outcomes, then the equilibrium contribution schedule cannot be monotonic. In particular, while there is no equilibrium in which a contribution fully reveals the group's ideal point, there can be equilibria in which the contribution schedule is stepwise U-shaped. In such an equilibrium, a group with the most extreme ideal point gives the same contribution as a group with the same ideal point as the legislator. Before presenting these results (among others) formally, it is useful to give some intuition for why they obtain with a numerical example.

FIGURE 3

Equilibrium Contributions in Example 1



$X = [0,1]$;
$X(0.041) = \{0\} \cup [0.526,1]$;
$X(0) = (0,0.526)$.

NOTE: Equilibrium requires $\begin{cases} qW(x;0.041,0) \geq 0.041 \text{ for all } x \in X(0.041) \\ qW(x;0.041,0) < 0.041 \text{ for all } x \in X(0) \end{cases}$

EXAMPLE 1.[9] *Assume:* $X = [0, 1]$; $F(x) = .2 + \dfrac{x}{1.25}$; $q = .9$; *and* $k(c) = .055 - c$. *Then (to 3 decimal places), there is an equilibrium in which*

i. the group gives a contribution of $\gamma^*(x) = .041$ *if* $x = 0$ or $x \in [.526, 1]$, *and the group gives nothing otherwise;*

ii. that legislator grants access only who receives a contribution of at least .041 (and if the group makes no contribution it offers no speech);

iii. given access is granted and the issue is legislatively relevant, if the group has ideal point $x = 0$ *and has observed a value of* $t < .364$ *it makes a speech* m_1 *(e.g., "the value of* t *is low"), otherwise it makes a speech* m_2 *(e.g., "the value of* t *is high"); on the other hand, if the group has ideal point* $x \in [.526, 1]$, *it makes the speech* m_2 *irrespective of the observed value of* t; *and*

iv. if access is not granted and the issue is legislatively relevant, the legislator takes the action $\delta^*(0, \emptyset) = .5$; *if access is granted and the legislator hears the speech* m_1, *he or she takes the action* $\delta^*(.041, m_1) = .182$, *and if the speech* m_2, *takes the action* $\delta^*(.041, m_2) = .546$.

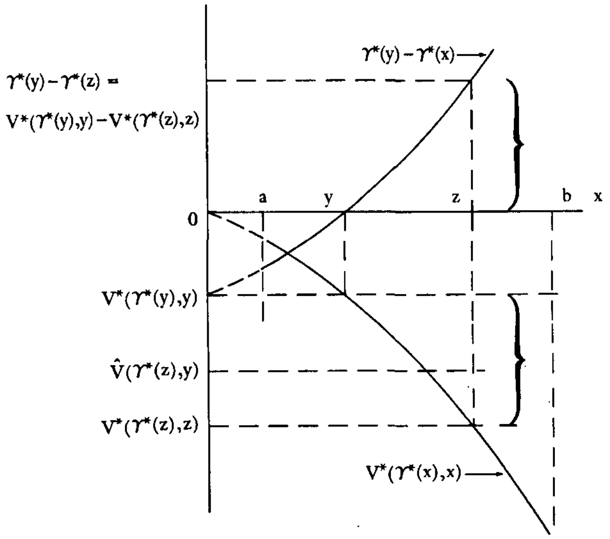
Figure 3 illustrates the equilibrium contribution schedule (ignore the graph labeled $W(x; .041, 0)$ for the moment). The first thing to note about this equilibrium is that despite the contribution schedule being U-shaped, the conditional expectation on the ideal point of the group when the contribution is zero strictly exceeds that when the contribution is .041; specifically (to three decimal places), $E[x|c = .041] = .237 < E[x|c = 0] = .263$. So the statistical observation is that groups with ideal points closer to those of the legislator typically give more money to the legislator than do groups with more disparate ideal points. Second, although there is an influential equilibrium available if the legislator grants access when receiving no contribution, the anticipated payoff is insufficient to make granting access profitable.

Now consider the intuition behind the equilibrium of example 1, beginning with the contribution schedule. If the contribution schedule γ^* fully reveals the group's ideal point, it is said to be *separating;* and for every distinct pair of ideal points x and y, $\gamma^*(x) \neq \gamma^*(y)$. In the present context, if $\bar{x} > .25$, then γ^* trivially cannot be separating over all of X. This is because, given that γ^* is separating and the group's ideal point is known for sure, there is no informational gain to the legislator of granting access when this ideal point is .25 or above (Crawford and Sobel 1982); in effect, the group's and the legislator's preferences over final outcomes are so disparate that no persuasive communication can take place. Consequently no group with an ideal point in this range will make a positive contribution that reveals its preferences unequivocally. On the other hand, γ^* might be separating over some interval of group ideal points lying within .25 of the legislator's ideal point. And if γ^* could separate over $[0, .25)$, then γ^* would be separating over all of X *with respect to the value of access*. Unfortunately, as claimed earlier, no such separation is possible in equilibrium. The reason is that to support separation over any interval, the contribution schedule must decrease in x "too fast" to deter a group with an ideal point relatively close to that of the legislator from mimicking the behavior of a group with an ideal point slightly further away. In effect, the group's expected loss in the value of access from such mimicry is smaller than the gain it derives by paying less for access.[10]

More specifically, assume to the contrary that γ^* is separating over some interval $I \subset [0, .25)$ and let $V^*(\gamma^*(x), x)$ denote the expected payoff (net of contribution) of access to a group with ideal point x in I. Given that γ^* is separating, it must be differentiable almost everywhere on I; so let γ^* be differentiable on $(a, b) \subset I$. Let y and z be ideal points in (a, b) with $y < z$, and consider Figure 4. The graph $V^*(\gamma^*(x), x)$ describes how the expected value of access changes as x increases over (a, b), given that γ^* is separating over the interval. Since $V^*(\gamma^*(x), x)$ is strictly decreasing in x, $\gamma^*(x)$ must likewise be strictly decreasing in x. Furthermore, it turns out to be necessary that for any two ideal points like y and z, the difference in the respective expected contributions must be exactly equal to q times the difference in the respective values of access. Hence, as illustrated in the figure, $\gamma^*(y) - \gamma^*(x)$ has the shape drawn and, for instance, if $q = 1$ then $\gamma^*(y) - \gamma^*(z)$ must equal the difference in expected payoffs, $V^*(\gamma^*(y), y) - V^*(\gamma^*(z), z)$. However, if a group with ideal point y makes the contribution $\gamma^*(z)$ and subsequently mimics the equilibrium lobbying behavior of a group with ideal point z, its expected payoff, say $\hat{V}(\gamma^*(z), y)$, lies strictly between $V^*(\gamma^*(z), z)$ and $V^*(\gamma^*(y), y)$. Therefore, the loss in payoff incurred by the group with ideal point y by mimicking the lobbying behavior of a group with

FIGURE 4

Illustration of the Argument for No Separating Equilibrium (Assuming q = 1)



ideal point z is strictly smaller than the gain it accrues by making the smaller contribution, $\gamma^*(z)$, rather than $\gamma^*(y)$. Consequently, the supposition that γ^* is separating on (a, b) is not tenable in equilibrium.

It follows that if the contribution schedule reveals anything to the legislator, then it must be some sort of step function under which the contribution given identifies a range of possible ideal points—strictly contained in X—that includes the group's true ideal point. When $\bar{x}$ is sufficiently large (bigger than 1 is sufficient but by no means necessary) the only possible such functions that can occur in equilibrium are nonmonotonic; in example 1, the schedule is U-shaped. As illustrated by example 1, a group with the most extreme ideal point will make the same contribution as a group whose ideal point coincides with that of the legislator; on receiving the contribution, therefore, the legislator is better informed regarding the group's preferences but still not perfectly informed. That such a *semipooling* contribution strategy should be stepwise decreasing over low ranges of x is intuitive; as repeatedly observed, the expected value of access to both group and legislator is decreasing in the extent to which their preferences diverge. The surprise is that the schedule must then be stepwise increasing for sufficiently high ranges of x.

To see how the U shape occurs, recall that for any observation of t, the higher x is, the higher the action the group would like to see taken by the legislator. So a group with sufficiently extreme preferences (high x) always encourages the legislator to take the largest possible action, irrespective of the true value of t. Moreover, since the group is risk-averse, the utility difference it experiences between having the largest and the second largest elicitable action taken is strictly increasing in x. Consequently, if the largest elicitable action is available only if the legislator has some particular beliefs about the group's ideal point induced from its contribution, then, for sufficiently high values of x, the group is willing to make the relevant contribution to induce these beliefs. Finally, since a group with ideal point close to the legislator is ceteris paribus more credible for the legislator, it is such a group that has the most influence and is capable of inducing the highest actions. Hence, there is an incentive for a group with an extreme ideal point to mimic the contribution behavior of a group with an ideal point closer to that of the legislator.

For example 1, the relevant difference in expected payoffs is graphed in Figure 3 as $W(x; .041, 0) = [V^*(.041, x) - V^*(0, x)]$. The first term of $W(\cdot)$ is the expected payoff to a group with ideal point x if that group lobbies the legislator when the legislator believes the group has ideal point $x \in \{0\} \cup [.526, 1]$. Thus such a group can gain access and either make speech m_1 to elicit an action of .182, or make speech m_2 and elicit the action .546. The second term of $W(\cdot)$ is the expected payoff if no contribution is made and the legislator optimally responds by not granting access and simply choosing the action .5. When x is greater than the midpoint between .182 and .546 then, conditional on being granted access, *whatever* the value of t the group observes, the group's best available lobbying strategy is to make the speech m_2 and elicit .546. Since this is strictly greater than the action forthcoming if there is no lobbying by the group (in which case, the legislator chooses action .5) and the group is risk-averse, the expected payoff difference $W(x; .041, 0)$ is strictly increasing in x for x sufficiently large. Therefore, whatever contribution $x = 0$ is willing to pay to secure access, there is a sufficiently extreme ideal point such that a group with this ideal point is willing to make the same contribution. As illustrated in Figure 3, then, a group with ideal point $x = .526$ is just indifferent between contributing and so gaining access (conditional on the issue becoming relevant), and not contributing.

Now assume access is granted when $c = .041$ and consider the agents' behavior at the lobbying stage. First, let a_1 and a_2 be the two actions elicited by, respectively, speeches m_1 and m_2, with $a_1 < a_2$. Then any group with *induced* ideal point $x + t$ less (greater) than $(a_1 + a_2)/2$ will make the speech m_1 (m_2). This follows from the usual spatial voting logic with symmetric preferences: elicit the alternative that is closest to your ideal point. Then taking account of this lobbying behavior by the group, the legislator chooses the actions that maximize the expected payoff. In the equilibrium, the actions a_1 and a_2 so chosen must be consistent with the group's behavior and conversely.

In example 1, the actions a_1 and a_2 are, respectively, .182 and .546; so $(a_1 + a_2)/2$ is .364. Since x is either zero or between .526 and 1, the preceding discussion implies that the only sort of group who would send m_1 is a group with $x = 0$; hence, the conditional expectation on t, given m_1 and this information, is that $t = .182$. Since the legislator's optimal response is to choose the action that equals the expected value of t, this calculation results in precisely the action elicited by m_1. The legislator's decision calculus is a little more complicated when he or she hears m_2. In this instance, the information on the group's preferences is much less precise. But using this information and the group's lobbying rule yields a conditional expectation of $t = .546$, again the action elicited by m_2.[11]

EQUILIBRIUM

I shall describe the formal results, discussed for the most part through example 1. The lobbying subgame, played conditional on the issue being relevant and the legislator granting access, is essentially the canonic Sender/Receiver cheap-talk game introduced by Crawford and Sobel (1982). The one difference here is that the legislator (Receiver) may be uncertain about the group's (Sender's) preferences over the consequences of actions. Because the willingness of the legislator to grant access depends entirely on the expected (informational) value from listening to the group's lobbying speech, it is necessary first to identify the circumstances under which there can exist an influential equilibrium to the lobbying subgame.

Let $X(c) \subseteq X$ denote the support of the legislator's posterior beliefs about the parameter x, given contribution $c \geq 0$, at the start of the lobbying subgame.

PROPOSITION 1. *Let* $f(\cdot|c)$ *denote the legislator's posterior density function over* $X(c)$. *Then there exists an influential equilibrium to the lobbying subgame*

i. if $\int_{-1/4}^{1/4} f(x|c)\,dx > 0$; *and*
ii. only if $\int_{-1/2}^{1/2} f(x|c)\,dx > 0$.

Thus, if the legislator's beliefs over the group's ideal point x, following the campaign contribution c, put positive weight on x being within a distance of 1/4 from her own ideal point, then there surely exists an influential equilibrium to the lobbying subgame, and granting access can lead to informational gains for the legislator. Of course, these gains may not be sufficient to warrant bearing the cost, $k(c)$, of granting access at all. Indeed, as the proof to the result makes apparent, the maximal value of access is increasing in the extent to which the weight of the legislator's posterior beliefs, $F(\cdot|c)$, is concentrated on low values of $|x|$. In other words, the more confident the legislator that the group's ideal point is close to his or her own (at zero), the larger the anticipated value of access.

Let (λ^*, δ^*) be the lobbying subgame strategies for some specified equilibrium. Let $U^*(\lambda^*, \delta^*; c)$ denote the legislator's ex ante expected payoff from (λ^*, δ^*), given a campaign contribution c and given the legislator grants access. To save on notation, when there is no ambiguity about the structure of (λ^*, δ^*), I shall write $U^*(\lambda^*, \delta^*, c) \equiv U^*(c)$ if and only if $\lambda^* \neq \varnothing$ and, throughout, write $U^*(\varnothing, \delta^*; c) \equiv U^\circ(c)$. Then the legislator will grant access if and only if he or she expects to benefit by so doing:

$$\alpha^*(c) > 0 \Leftrightarrow U^*(c) - k(c) \geq U^\circ(c). \quad (3)$$

Let $V^*(\lambda^*, \delta^*; c, x)$ denote the type-x group's ex ante expected payoff from the specified lobbying subgame equilibrium, given a contribution of c. As for the case of the legislator, when there is no ambiguity about (λ^*, δ^*), I shall write $V^*(\lambda^*, \delta^*; c, x) \equiv V^*(c, x)$ if and only if $\lambda^* \neq \varnothing$ and write $V^*(\varnothing, \delta^*; c, x) \equiv V^\circ(c, x)$ throughout. Because campaign contributions have to be made prior to Nature choosing (with probability q) whether the policy issue becomes legislatively relevant, we have

$$\begin{aligned} \gamma^*(x) = c > 0 \Leftrightarrow\ & q[\alpha^*(c)V^*(c, x) \\ & + (1 - \alpha^*(c))V^\circ(0, x)] - c \geq qV^\circ(0, x) \\ \Leftrightarrow\ & q\alpha^*(c)[V^*(c, x) - V^\circ(0, x)] - c \geq 0. \end{aligned} \quad (4)$$

Evidently, no group makes an equilibrium contribution unless there is positive probability of being granted access to the legislator. The converse statement is not necessarily true. For sufficiently low cost of access, $k(0)$, the fact that a group is informed might induce access to be granted even though no campaign contribution is made.

In equilibrium, strategies are required to be subgame perfect. Because contribution strategies are pure, this implies that if $c > 0$ and the legislator is indifferent between granting and not granting access, he or she must grant access with probability 1. Consequently, $\alpha^*(c) \in \{0, 1\}$ always, and we can set $\alpha^*(c) = 1$ in expression 4.

The focus of concern here is on the signaling role of campaign contributions in securing access to legislators. There are three possibilities in this respect: (1) the equilibrium contribution strategy $\gamma^*(\cdot)$ might be *pooling*, in which $\gamma^*(x) = \gamma^*(x')$ all x, x' and no information regarding x (and therefore the value of access) be transmitted; (2) the strategy might be *separating* over some interval, in which $\gamma^*(x) \neq \gamma^*(x')$ all $x \neq x'$ in the interval and the true value of x be completely revealed; and (3) the strategy might be *semipooling*, in which $\gamma^*(x) \neq \gamma^*(x')$ for some but not all $x \neq x'$, and some information be signaled. Consider these in turn.

Pooling Equilibria

As remarked earlier, there is always an equilibrium to the lobbying subgame in which no information is transmitted. Consequently, for all parameterizations of the model, there exists a pooling equilibrium in which no contributions are given, access is never granted, and the legislator chooses an action (if necessary) on the basis of prior beliefs alone. For pooling equilibria with a positive contribution to exist, the legislator's prior beliefs over X must concentrate around zero sufficiently to admit a lobbying equilibrium with high expected value of access to the legislator (given these beliefs) and the group, whatever its ideal point. Unfortunately, a general characterization of when such equilibria exist is not possible, depending as it does on the details of the distribution function $F(\cdot)$. However, if there does exist an equilibrium to the lobbying subgame sufficient to support an equilibrium contribution schedule γ^* in which $\gamma^*(x) = c > 0$ for all x, then there will almost always exist a continuum of pooling equilibria across which only the contribution offered varies. That is, if (λ^*, δ^*) is such an equilibrium to the lobbying subgame, then expressions 3 and 4 and the fact that the legislator's posterior and prior beliefs about x are the same (by γ^* pooling) directly imply that there is a continuum of equilibria involving (λ^*, δ^*) in which, for all $x \in X$, $\gamma^*(x) = c$ and

$$k^{-1}(U^*(0) - U^\circ(0)) < c < \min_{x} q(V^*(0, x) - V^\circ(0, x)). \quad \textbf{(5)}$$

If c is less than the lower bound in expression 5, the legislator will not be willing to grant access when (λ^*, δ^*) characterizes the subsequent lobbying stage; and if c exceeds the upper bound in expression 5, the group with the lowest valuation of (λ^*, δ^*) will not be willing to make the contribution. Any common contribution between these bounds is consistent with equilibrium behavior, conditional on (λ^*, δ^*).

In these sort of pooling equilibria, contributions are made and access is granted although the legislator's beliefs about the group's preferences are unaffected by any giving. Thus contributions serve only to lower the cost of granting access and fail to discriminate among groups. Other things being equal, that is, the legislator would choose to grant access to the group irrespective of any financial incentive; insofar as the contribution matters, therefore, it is only because access is costly to grant for the legislator. In particular, for a sufficiently low (positive) cost of access, access will surely be granted irrespective of any contribution.

The situation is consistent with some of the descriptive statements about the access role of money recorded by Sabato (1985), Shlozman and Tierney (1986), and others. However, since all types of group give the same contribution in equilibrium, the situation is inconsistent both with the positive correlation between contributions and the extent of access (Langbein 1986) and with the positive correlation between contributions and preference similarity (e.g., McCarty and Poole 1993).

Separating Equilibria

Separating equilibria and the intuition behind the result are discussed in some detail in the context of example 1. It therefore suffices to state the claim formally and refer to the earlier discussion for the intuition.

PROPOSITION 2. *Let* $Y \subseteq X \cap (0, \bar{x})$ *be a nondegenerate interval. Then there exists no equilibrium in which* γ^* *is separating on* Y.

By symmetry, the same result holds for any nondegenerate interval $Y \subseteq X \cap (\underline{x}, 0)$. Hence, proposition 2 implies that campaign contributions are incapable of unequivocally signaling a group's preferences whenever the set of possible ideal points is not discrete. This result holds even when the legislator knows ex ante that the group's ideal point is (say) strictly positive.

Semipooling Equilibria

In view of proposition 2, if there are any informative contribution strategies in equilibrium then they must be semipooling. When the group's ideal point, x, is common knowledge, the expected value of the most influential equilibrium to both group and legislator is decreasing in the distance between their respective preferences; (i.e., decreasing in $|x|$). This suggests that if γ^* is semipooling, then it will have a partition structure in which groups with ideal points closer to that of the legislator make higher contributions than those with relatively extreme ideal points. However, a necessary but not sufficient condition for this is that the support of F is not "too big."

PROPOSITION 3. *Assume* $X = [0, \bar{x})$ *with* $\bar{x} > 1$. *If there is an equilibrium with* γ^* *semipooling then* γ^* *cannot be (stepwise) monotonic on* X. *In particular, if exactly one equilibrium contribution induces access, then* γ^* *semipooling implies* γ^* *is (stepwise) U-shaped on* X.

The assumption that $X = [0, \bar{x})$ is not essential to the nonmonotonicity result. However, when $X = (-\underline{x}, \bar{x})$, say, it becomes harder for the contribution schedule to reveal any preference information. This is because pooling by the extreme types generally drives the legislator's actions in any lobbying subgame to the action he or she would take without lobbying, which in turn lowers the expected value of access to the legislator and any group.

The intuition for proposition 3 is discussed at length in the context of example 1 and need not be rehearsed again here. And note that example 1 also demonstrates that semipooling equilibria of the sort identified in proposition 3 can exist.

Putting the propositions together, the result is that if campaign contributions promote access insofar as they signal information about the value of access, then the signal is necessarily noisy and the value of access to the legislator—and to groups close to the legislator—is accordingly depressed.

DISCUSSION

Under complete information about an interest group's preferences over legislative decisions, the canonic information-based story connecting campaign contributions to access predicts, as illustrated in Figure 1, a positive relationship between campaign contributions and preference disparity between donor and recipient. However, one of the more robust empirical findings in the literature is that this relationship is negative. I have argued that the prediction of the canonic story is due largely to the complete information assumption on groups' preferences. So rather than reject the access story out of hand, within the context of a simple model I reexamine the argument without this assumption. In this environment, contributions can in principle provide a signal of the value of access to the legislator, in which case the observed empirical pattern of contributions could be generated, since the expected value of access is prima facie increasing in the extent to which the legislator's and the group's preferences are similar.

The results suggest that when contributions do provide some information on group preferences and so induce access when none would otherwise be forthcoming, they do so only noisily. In particular, if the possible ideal points of groups are sufficiently widely dispersed, groups with preferences that very closely reflect those of the legislator will make the same contribution as groups whose preferences are very distant from those of the legislator. Consequently, while access can be induced by such contributions, the extent to which information can be transmitted through the lobbying process is attenuated and the legislator always faces the prospect of listening to a group with essentially state-independent preferences. On the other hand, the prospect of facing a group with essentially state-independent preferences is small. When contributions are informative the statistical expectation is typically that the preferences of groups granted access are close to those of the legislator. Thus, as conjectured, the model is capable of generating a positive observed relationship between preference similarity and contributions.

The preceding claims are predicated on the assumption that contributions are given before the group becomes asymmetrically informed relative to the legislator. In the model, this is motivated by the descriptive stories describing securing access as securing an option to talk to a legislator should the need arise and by the empirical fact that most details of legislative agendas only emerge over the course of the legislative session. However, it turns out that the particular sequencing of decisions is substantively important. It can be shown within the model that if groups are known to be informed prior to making any contribution, gaining access for purposes of information transmission is irrelevant; the equilibrium contribution schedule per se is capable of revealing all of the information that can be credibly revealed.[12] In other words, for contributions to secure access purely for purposes of informational lobbying, legislators must at least be unsure about whether a donor is well informed on the particular issue of concern at the time of the contribution. If this is not the case and if granting access has substantive value for either the legislator or the group, then such value is not derived from any policy-relevant information transmission. However, whether or not this result holds when a group is known to have information on multiple issues but Nature selects, say, at most one of these as legislatively relevant in any given session, is unclear.

The model here includes electoral considerations only implicitly; most importantly, the decision concerning which candidate(s) to support is taken as given. When this decision is explicitly introduced a variety of additional issues arise (McCarty and Rothenberg 1993). In particular, the decision to invest in securing access will be influenced by the probability of any candidate's electoral success. Under such circumstances, it is quite possible to find money and access correlated but not directly connected in any quid pro quo sense. As argued earlier, if information is valuable, a legislator will seek data from those informed agents whose preferences most closely reflect his or her own. Knowing this, interest groups prefer legislatures consisting of more people with preferences close to their own to legislatures with fewer such people; therefore, groups give money to candidates who are relatively like themselves to increase the probability of such legislatures arising. De facto, therefore, to the extent that money influences electoral outcomes, it will appear that money buys access.

Two further caveats are worth making. First, there is only one group in the model, and it is known that in strategic information transmission models of lobbying the presence of competing groups can affect groups' lobbying strategies (Austen-Smith and Wright 1994; Gilligan and Krehbiel 1989). Second, the model here is "one-shot," thus precluding reputational effects. Future work should address at least these issues.

APPENDIX

Definition. A list of strategies, $\sigma^* \equiv (\gamma^*, \alpha^*, \lambda^*, \delta^*)$, and a set of beliefs constitute an equilibrium if

$\forall x \in X,\ \forall c,$

$$E[v(\delta^*(\gamma^*(x), m), t; x) - \gamma^*(x) | \gamma^*(x), \alpha^*, \lambda^*] \geq E[v(\delta^*(c, m), t; x) - c | c, \alpha^*, \lambda^*] \quad \textbf{(A-1}\textit{a}\textbf{)}$$

$\forall c \in \mathbb{R}_+,\ \alpha^*(c) > 0$ iff,

$$E[u(\delta^*(c, m), t) - k(c) | c, \lambda^*] \geq E[u(\delta^*(c, \emptyset), t) | c, \emptyset]$$

$\forall (x, c, t) \in X \times \mathbb{R}_+ \times [0, 1],\ \forall \lambda,$

$$v(\delta^*(c, \lambda^*(x, t)), t; x) \geq v(\delta^*(c, \lambda(x, t)), t; x) \quad \textbf{(A-1}\textit{b}\textbf{)}$$

$\forall (c, m) \in \mathbb{R}_+ \times M,\ \forall \delta,$

$$E[u(\delta^*(c, m), t) | c, m] \geq E[u(\delta, t) | c, m] \quad \textbf{(A-2)}$$

and if beliefs at every stage are derived from σ^* and the priors using Bayes Rule, where this is defined.

In particular, the legislator's beliefs over $x \in X$ conditional on receiving a contribution c, $f(x|c)$, and over $t \in [0, 1]$ conditional on receiving a contribution c and hearing a message m, $g(t|c, m)$, are defined as follows. Let $\gamma(c|x)$ = Pr[c contributed given x] and $\lambda(m|x, t)$ = Pr[m sent given (x, t)]. Then,

$$f(x|c) = \frac{f(x)\gamma(c|x)}{\int f(x)\gamma(c|x)\,dx},$$

when the denominator is positive

$$g(t|c, m) = \frac{g(t) \int \lambda(m|x, t) f(x|c)\,dx}{\int g(t) \int \lambda(m|x, t) f(x|c)\,dx\,dt},$$

when the denominator is positive. **(A-3)**

Remark 1. Because contributions have to be given before a group learns (if it ever does) the true value of t, the legislator's beliefs over $t \in [0, 1]$ at the start of the lobbying subgame are given by the prior over t (i.e., $g(t) = 1\ \forall t \in [0, 1]$).

The first result, Lemma 1, characterizes influential equilibria to the lobbying subgame. Let $X(c) \subseteq X$ be the support of $f(\cdot|c)$ and, for any message $m \in M$ and contribution $c \in \mathbb{R}_+$, let $f(\cdot|c, m)$ denote the legislator's posterior density over X conditional on (c, m).

LEMMA 1. *In any influential equilibrium* (λ^*, δ^*) *to the lobbying subgame,*

1. $\forall x \in X(c)$, $\lambda^*(x, \cdot)$ *is characterized by a partition on* $[0, 1]$, $\langle t_0(x) \equiv 0, t_1(x), \ldots, t_{N(x)-1}(x), t_{N(x)}(x) \equiv 1\rangle$ *such that*
   - *i.* $\forall i = 1, \ldots, N(x)$, $\forall s \in (t_{i-1}(x), t_i(x)]$, $\lambda^*(x, s) = m_i$ *with* m_i *distinct across* i;
   - *ii.* $\forall i = 1, \ldots, N(x) - 1$, $t_i(x) = [\delta^*(c, m_i) + \delta^*(c, m_{i+1})]/2 - x$.
2. $\forall m_i \in \cup_{X(c)} [\cup_{s=0}^{s=1} \lambda^*(x, s)]$, $\delta^*(c, m_1) = \int_0^1 tg(t|c, m_i)\,dt$.

Proof. Let $x \in X(c)$. Given $\delta^*(\cdot)$, equation A-1b and the fact that both the group and the legislator use pure strategies in the lobbying subgame imply $\forall s, t \in [0, 1]$:

$$v(\delta^*(c, \lambda^*(x, t)), t; x) \geq v(\delta^*(c, \lambda^*(x, s)), t; x) \quad \textbf{(A-4)}$$

$$v(\delta^*(c, \lambda^*(x, s)), s; x) \geq v(\delta^*(c, \lambda^*(x, t)), s; x). \quad \textbf{(A-5)}$$

Without loss of generality, let $t > s$; and write $\lambda^*(x, t) = m$ and $\lambda^*(x, s) = m'$. Using equation 2, A-4 and A-5 are then equivalent to

$$2(x + t)[\delta^*(c, m) - \delta^*(c, m')] \geq \delta^*(c, m)^2 - \delta^*(c, m')^2$$

$$2(x + s)[\delta^*(c, m) - \delta^*(c, m')] \leq \delta^*(c, m)^2 - \delta^*(c, m')^2.$$

Hence, $[\delta^*(c, m) - \delta^*(c, m')](t - s) \geq 0$ in which case $\delta^*(c, m) \geq \delta^*(c, m')$. Moreover, $\exists t(x, m, m')$ such that for $\delta^*(c, m) > \delta^*(c, m')$ we have

$$\forall x \geq 0,\ t(x, m, m') = \max.\{0, [\delta^*(c, m) + \delta^*(c, m')]/2 - x\};$$

$$\forall x < 0,\ t(x, m, m') = \min.\{[\delta^*(c, m) + \delta^*(c, m')]/2 - x, 1\};\ \text{and}$$

$$\forall s < t(x, m, m'),\ v(\delta^*(c, m'), s; x) > v(\delta^*(c, m), s; x)$$

$$\forall s > t(x, m, m'),\ v(\delta^*(c, m'), s; x) < v(\delta^*(c, m), s; x).$$

Claim 1.1 follows.

Now fix $\lambda^*(x, t)$. By equations 1 and A-2, for any

$$m_i \in \bigcup_{X(c)} \left[\bigcup_{s=0}^{s=1} \lambda^*(x, s)\right],$$

$$\delta^*(c, m_i) = \underset{b \in \mathbb{R}}{\text{argmax.}} - (E[s|c, m_i] - b)^2 - \text{var}[s|c, m_i]$$

$$= E[s|c, m_i]$$

$$= \int_0^1 tg(t|c, m_i)\,dt.$$

Q.E.D.

Remark 2. Suppose the support of the legislator's posterior over X conditional on receiving a contribution c is singleton; that is, the contribution strategy γ fully reveals the group's ideal point, say, $X(c) = \{x\}$. Then Lemma 1 and $g(t) = 1\ \forall t \in [0, 1]$ imply

$$E[s|c, m_i] = \int_0^1 tg(t|c, m_i)\,dt = \frac{[t_{i-1}(x) + t_i(x)]}{2}.$$

Proof of Proposition 1 (Sufficiency). By Lemma 1.1 and 1.2, if there is any influential equilibrium to the lobbying subgame, there is an equilibrium in which exactly two actions are elicited; say, $0 \leq a < b \leq 1$. Let inf.$X(c) = \underline{x}(c)$; sup.$X(c) = \bar{x}(c)$; and $y(a, b) = [a + b]/2$. When there is no ambiguity, the dependency of y on (a, b), and of $\bar{x}$ and $\underline{x}$ on c, will be suppressed. Given $a < b$, let λ be a message strategy for the lobbying subgame such that

$$\lambda(m_1|x, t) = 1 \text{ iff } t \leq y - x$$

$$\lambda(m_2|x, t) = 1 \text{ iff } t > y - x.$$

By equation A-3 and $g(t) = 1\ \forall t \in [0, 1]$, therefore, assuming for the moment that $g(t|c, m_1)$ is well defined,

$$E[t|c, m_1, y] = \int_0^1 tg(t|c, m_1)\,dt$$

$$= \int_0^1 t.\frac{\int \lambda(m_1|x, t) f(x|c)\,dx}{\int\int \lambda(m_1|x, t) f(x|c)\,dx\,dt}\,dt$$

$$= \int_0^1 t.\varphi(y - t|c)\,dt \Big/ \left[\int_0^1 \varphi(y - t|c)\,dt\right] \quad \textbf{(A-6)}$$

where

$$\varphi(y-t|c)=\begin{cases}F(y-t|c)\text{ if }\underline{x}<y-t<\bar{x}\\ 1\text{ if }y-t\geq\bar{x}\\ 0\text{ if }y-t\leq\underline{x}.\end{cases}\qquad\textbf{(A-7)}$$

Trivially, $E[t|c, m_1, y] \geq 0$. To see that $E[t|c, m_1, y] \leq 1/2$ also, suppose not; then

$$\int_0^1 t.\varphi(y-t|c)\,dt\Big/\left[\int_0^1 \varphi(y-t|c)\,dt\right] > 1/2$$

$$\Leftrightarrow \int_0^1 (2t-1)\varphi(y-t|c)\,dt > 0$$

$$\Leftrightarrow \int_0^{1/2} (1-2t)\varphi(y-t|c)\,dt$$

$$< \int_{1/2}^1 (2t-1)\varphi(y-t|c)\,dt. \qquad\textbf{(A-8)}$$

By equation A-7, $t > t'$ implies $\varphi(y-t|c) \leq \varphi(y-t'|c)$; moreover, there is a one-to-one and onto map ρ: $[0, 1/2] \rightarrow [1/2, 1]$ such that $\forall t \in [0, 1/2]$, $1-2t = 2\rho(t) - 1$. Hence, $\forall t \in [0, 1/2]$, $(1-2t)\varphi(y-t|c) \geq (2\rho(t)-1)\varphi(y-\rho(t)|c)$, and therefore,

$$\int_0^{1/2} (1-2t)\varphi(y-t|c)\,dt \geq \int_0^{1/2} (2\rho(t)-1)\varphi(y-\rho(t)|c)\,dt = \int_{1/2}^1 (2t-1)\varphi(y-t|c)\,dt,$$

contradicting the supposition. So $E[t|c, m_1, y] \in [0, 1/2]$ $\forall (a, b) \in [0, 1]^2$ with $a < b$. Similarly, derive

$$E[t|c, m_2, y] = \int_0^1 tg(t|c, m_2)\,dt$$

$$= \int_0^1 t.\frac{\int \lambda(m_2|x, t)f(x|c)\,dx}{\int\int \lambda(m_2|x, t)f(x|c)\,dx\,dt}\,dt$$

$$= \int_0^1 t.[1-\varphi(y-t|c)]\,dt\Big/\left[\int_0^1 [1-\varphi(y-t|c)]\,dt\right], \qquad\textbf{(A-9)}$$

and deduce $E[t|c, m_2, y] \in [1/2, 1]$ for all $a < b$. Now define the mapping h: $[0, 1/2] \times [1/2, 1] \rightarrow [0, 1/2] \times [1/2, 1]$ by

$$\forall (a, b) \in [0, 1/2] \times [1/2, 1],\ h(a, b) = (E[t|c, m_1, y], E[t|c, m_2, y]).$$

Under the assumption that $\int_{-1/4}^{1/4} f(x|c)\,dx > 0$, the mapping h is continuous. To show this, it suffices to check that for $m \in \{m_1, m_2\}$, $E[t|c, m, y]$ is continuous in $y = [a+b]/2$, $(a, b) \in [0, 1/2] \times [1/2, 1]$. By definition, $y \in [1/4, 3/4]$. Now let $\mathcal{F}(y|c) = \int_0^1 \varphi(y-t|c)\,dt$ and note $\mathcal{F}(y|c) = 0$ only if $\varphi(y-t|c) = 0$ $\forall t \in [0, 1]$. By equation A-7, this can occur only if $y \leq \underline{x}$; but since $y \geq 1/4$, this implies $\underline{x} \geq 1/4$, which contradicts the assumption

$$\int_{-1/4}^{1/4} f(x|c)\,dx > 0.$$

So $\forall (a, b)$ in the domain of h, $E[t|c, m_1, y]$ is well defined. Moreover, by definition of φ and F continuous, $\mathcal{F}(\cdot|c)$ is continuous in y. And since φ is continuous, $E[t|c, m_1, y]$ is continuous as required. From equations A-6 and A-9,

$$E[t|c, m_2, y]$$

$$= \int_0^1 t.[1-\varphi(y-t|c)]\,dt\Big/\left[\int_0^1 [1-\varphi(y-t|c)]\,dt\right]$$

$$= \frac{1-2\mathcal{F}(y|c)E[t|c, m_1, y]}{2[1-\mathcal{F}(y|c)]}.$$

Therefore, $E[t|c, m_2, y]$ is continuous in y so long as $\mathcal{F}(y|c) < 1$ $\forall y$. If $\mathcal{F}(y|c) = 1$, then necessarily $\varphi(y-t|c) = 1$ $\forall t \in [0, 1]$. By equation A-7, this can only occur if $y - 1 \geq \bar{x}$; but since $y \leq 3/4$ this implies $\bar{x} \leq -1/4$, contradicting the assumption $\int_{-1/4}^{1/4} f(x|c)\,dx > 0$. Hence h is a continuous function as claimed. It follows that h has a fixed point, say $(a^*, b^*) \in [0, 1/2] \times [1/2, 1]$. By construction and Lemma 1, (a^*, b^*) identifies an equilibrium (λ^*, δ^*) to the lobbying subgame in which

$$\begin{cases}\lambda^*(m_1|x, t) = 1 \text{ iff } t \leq [a^*+b^*]/2 - x;\\ \lambda^*(m_2|x, t) = 1 \text{ iff } t > [a^*+b^*]/2 - x;\\ \delta^*(c, m_1) = a^* \text{ and } \delta^*(c, m_2) = b^*.\end{cases}\qquad\textbf{(A-10)}$$

By definition, (λ^*, δ^*) will be influential iff $a^* < b^*$. By construction $a^* = b^*$ iff $a^* = 1/2$, implying $y^* = [a^* + b^*]/2 = 1/2$. By equation A-6, $a^* = E[t|c, m_1, y^*] = 1/2$ iff

$$\int_0^1 (2t-1)\varphi(y^*-t|c)\,dt = 0$$

$$\Leftrightarrow \int_0^{1/2} (1-2t)\varphi(y^*-t|c)\,dt$$

$$= \int_{1/2}^1 (2t-1)\varphi(y^*-t|c)\,dt. \qquad\textbf{(A-11)}$$

By equation A-7, $t > t'$ implies $\varphi(y^* - t|c) \leq \varphi(y^* - t'|c)$, and there is a one-to-one and onto map ρ: $[0, 1/2] \to [1/2, 1]$ such that $\forall t \in [0, 1/2]$, $1 - 2t = 2\rho(t) - 1$. By the same argument following equation A-8, therefore, equation A-11 is possible only if $\varphi(y^* - t|c) = \bar{\varphi}\ \forall t \in [0, 1]$. But since $y^* = 1/2$ and

$$\int_{-1/4}^{1/4} f(x|c)\,dx > 0$$

by assumption, this is impossible.

Proof of Proposition 1 (Necessity). If there is any influential equilibrium, then there is an equilibrium with only two elicited actions. So let (λ^*, δ^*) be an influential equilibrium to the lobbying subgame with $0 \leq a^* < b^* \leq 1$ being the only two actions elicited by λ^*. By Lemma 1, (λ^*, δ^*) is defined by A-10. Now suppose, contrary to the claim, that

$$\int_{-1/2}^{1/2} f(x|c)\,dx = 0.$$

By previous arguments, $a^* \leq 1/2$, $b^* \geq 1/2$, and $y^* = [a^* + b^*]/2 \in [1/4, 3/4]$. There are three cases: (1) $y^* = 1/2$, (2) $y^* > 1/2$, and (3) $y^* < 1/2$. Consider these in turn.

Case 1. By Lemma 1.1, $y^* = 1/2$ implies that $\forall t \in [0, 1]$:

$$\lambda^*(m_1|x, t) = 1\ \forall\, x < -1/2, \text{ and } \lambda^*(m_2|x, t) = 1\ \forall\, x > 1/2.$$

Therefore, since

$$\int_{1/2}^{1/2} f(x|c)\,dx = 0$$

by assumption, Lemma 1.2 implies:

$$a^* = E[t|c, m_1, y^*] = b^* = E[t|c, m_2, y^*] = 1/2,$$

contradicting $a^* < b^*$.

Case 2. Let $t_1 = y^* - (1/2)$. Since $y^* > 1/2$, $t_1 > 0$. By Lemma 1.1, $\forall t \in [0, 1]$:

$$\lambda^*(m_1|x, t) = 1\ \forall\, x < -1/2, \text{ and } \lambda^*(m_2|x, t) = 1\ \forall\, x > y^*.$$

Further,

$$\forall\, x \in (1/2, y^*],\ \lambda^*(m_2|x, t) = 1 \quad \forall\, t \in (y^* - x, 1].$$

Therefore, by Lemma 1.2 and

$$\int_{-1/2}^{1/2} f(x|c)\,dx = 0,\ b^* = E[t|c, m_2, y^*] \leq [1 + t_1]/2.$$

Substituting for t_1 and y^* and collecting terms gives $b^* \leq [1 + a^*]/3$. But since $a^* \leq 1/2$, this inequality implies $[a^* + b^*] \leq 1$, contradicting $y^* > 1/2$.

Case 3 follows from a similar argument as that for Case 2, filling in the details completes the proof.

Q.E.D.

Remark 3. Let $\{a_1, \ldots, a_N\}$ be the set of actions elicited in some influential equilibrium to the lobbying subgame following a contribution c; $0 < a_1 < \cdots < a_N < 1$. Let $y_1 = [a_1 + a_2]/2$ and $y_{N-1} = [a_{N-1} + a_N]/2$. Then replacing y by, y_1 in equation A-6 and y_{N-1} in equation A-9 defines a_1 and a_N, respectively.

Proof of Proposition 2. Suppose not; then γ^* is separating on some interval $Y \subseteq X \cap (0, \bar{x})$, in which case the lobbying subgame is exactly the canonic Crawford and Sobel (1982) strategic information transmission game. In particular, the group's ideal point is common knowledge prior to the group becoming informed (if at all) about the true value of $t \in [0, 1]$. For any $z \in Y$, let $\Lambda^*(z) \equiv (\lambda^*(z, \cdot), \delta^*(\cdot))$ denote the most influential lobbying equilibrium, conditional on z being common knowledge, in which the associated partition satisfying Lemma 1.1 is of size $N(z)$. By Lemma 1 and Remark 2, the legislator's posterior beliefs over $[0, 1]$ conditional on any message sent under $\lambda^*(z, \cdot)$ are uniform. By Crawford and Sobel 1982, Lemma 6, $\forall x, y \in Y$, $x > y$ implies $N(x) \leq N(y)$. By equation A-1a, $\forall x, y \in X$:

$$qV^*(\gamma^*(x), x) - \gamma^*(x) \geq qV^*(\gamma^*(y), x) - \gamma^*(y) \qquad \textbf{(A-12)}$$

$$qV^*(\gamma^*(y), y) - \gamma^*(y) \geq qV^*(\gamma^*(x), y) - \gamma^*(x), \qquad \textbf{(A-13)}$$

where $V^*(\gamma^*(x), x) \equiv E[v(\delta^*(\gamma^*(x), \lambda^*(x, t)), t, x)|\cdot]$ and $V^*(\gamma^*(x), y) \equiv E[v(\delta^*(\gamma^*(x), \lambda^*(y, t)), t, y)|\cdot]$, and so on. By 5, $\gamma^*(z) \neq 0$ only if $\Lambda^*(z)$ is influential; that is, $N(z) \geq 2$. Hence γ^* separating on Y implies $\gamma^*(z) > 0\ \forall z \in Y$. Now let $\hat{V}(\gamma^*(y), x)$ denote the expected payoff to the group with ideal point x from playing exactly the equilibrium strategies for the group with ideal point y; that is, $\hat{V}(\gamma^*(y), x) \equiv E[v(\delta^*(\gamma^*(y), \lambda^*(y, t)), t, x)|\cdot]$. Then by Lemma 1 and equation 2:

$$V^*(\gamma^*(x), x) - \hat{V}(\gamma^*(y), x) = \left[\sum_{i=1}^{i=N(y)} (t_i(y) - t_{i-1}(y))^3 - \sum_{i=1}^{i=N(x)} (t_i(x) - t_{i-1}(x))^3\right]/12.$$

Suppose $x > y$; then $N(x) \leq N(y)$ and, by Crawford and Sobel 1982, 1441, $\forall i = 1, \ldots, N(x)$, $(t_i(y) - t_{i-1}(y)) < (t_i(x) - t_{i-1}(x))$. Furthermore, $t_{N(\cdot)}(\cdot) = 1$ and $t_0(\cdot) = 0$. Therefore, $V^*(\gamma^*(x), x) - \hat{V}(\gamma^*(y), x) < 0$. So by definition of $V^*(\gamma^*(y), x)$, $V^*(\gamma^*(x), x) - V^*(\gamma^*(y), x) < 0$. Hence, A-12 implies $\gamma^*(x) < \gamma^*(y)$. Thus γ^* is strictly decreasing on Y and, therefore, γ^* is differentiable almost everywhere on Y (Royden 1968); in particular, there exists an open subinterval, $\hat{Y} \subset Y$, on which γ^* is differentiable. Then a necessary condition for A-12 and A-13 to hold is the following local incentive compatibility condition:

$$\forall\, x \in \hat{Y},\ d[qV^*(\gamma^*(x'), x) - \gamma^*(x')]/dx'|_{x'=x} = 0.$$

Therefore, for any $x < x'$, $x, x' \in \hat{Y}$,

$$q \int_{x}^{x'} [dV^*(\gamma^*(y), y)/\, dy]\, dy = \int_{x}^{x'} \gamma^{*\prime}(y)\, dy$$

$$\Rightarrow q[V^*(\gamma^*(x'), x') - V^*(\gamma^*(x), x)] = [\gamma^*(x') - \gamma^*(x)]. \quad \textbf{(A-14)}$$

With A-12 and A-13, therefore, equation A-14 implies $\forall x, y \in \hat{Y}$:

$$[V^*(\gamma^*(x), x) - V^*(\gamma^*(x), y)] \geq 0. \quad \textbf{(A-15)}$$

Consider any $x, y \in \hat{Y}$ with $x > y$. Recall that $\hat{V}(\gamma^*(x), y)$ is the expected payoff to the group with ideal point y from playing exactly the equilibrium strategies for the group with ideal point x, and $\forall i = 1, \ldots, N(x)$, let $\hat{t}_i(x) \equiv [t_i(x) + t_{i-1}(x)]/2$ and $\delta_i^* \equiv \delta^*(\gamma^*(x), m_i)$ where $\forall t \in (t_{i-1}(x), t_i(x)]$, $m_i = \lambda^*(x, t)$. Then Lemma 1 and equation 2 yield

$$[\hat{V}(\gamma^*(x), y) - V^*(\gamma^*(x), x)] = \sum_{i=1}^{i=N(x)} [t_i(x) - t_{i-1}(x)][(x + \hat{t}_i(x) - \delta_i^*)^2 - (y + \hat{t}_i(x) - \delta_i^*)^2] = (x^2 - y^2) + 2(x - y) \sum_{i=1}^{i=N(x)} [t_i(x) - t_{i-1}(x)][\hat{t}_i(x) - \delta_i^*].$$

Since γ^* is separating by supposition, Lemma 1 and Remark 2 imply $\delta^*(\gamma^*(x), m_i) = \hat{t}_i(x)$, all $i = 1, \ldots, N(x)$. Therefore, $[\hat{V}(\gamma^*(x), y) - V^*(\gamma^*(x), x)] = (x^2 - y^2) > 0$. But then, since $\hat{V}(\gamma^*(x), y) - V^*(\gamma^*(x), y) \leq 0$ by definition of $V^*(\cdot, y)$, $[V^*(\gamma^*(x), x) - V^*(\gamma^*(x), y)] < 0$: contradiction of A-15. Q.E.D.

LEMMA 2. *Let* c_a *be an equilibrium contribution such that the equilibrium message strategy in the consequent lobbying subgame elicits actions* $\{a_1, \ldots, a_A\}$, *where* $0 < a_1 < \cdots < a_A < 1$. *Similarly define a distinct equilibrium contribution* $c_b \neq c_a$ *and consequent elicited actions* $\{b_1, \ldots, b_B\}$, *with* $0 < b_1 < \cdots < b_B < 1$. *Then* $[V^*(c_a, x) - V^*(c_b, x)]$ *is continuous in* x *on* $[0, \infty)$, *differentiable in* x *except possibly at a finite number of values. Moreover, if* $B = 1$, *then* $[V^*(c_a, x) - V^*(c_b, x)]$ *can have at most one turning point.*

Proof. Let $t_i(x; c_a) = \max.\{0, [a_i + a_{i+1}]/2 - x\}$, $i = 1, \ldots, A - 1$; $t_0(x; c_a) = 0$ and $t_A(x; c_a) = 1$. Then by Lemma 1 and equation 2,

$$V^*(c_a, x) = \sum_{i=1}^{i=A} [t_i(x; c_a) - t_{i-1}(x; c_a)][(x + \hat{t}_i(x; c_a) - a_i)^2 + (t_i(x; c_a) - t_{i-1}(x; c_a))^2/12],$$

where $\hat{t}_i(x; c_a) = [t_i(x; c_a) + t_{i-1}(x; c_a)]/2$. Let $a(x)$ be the action elicited by the group paying c_a if the group observes $t = 0$; thus, $a(x) = a_A$ if $x \geq [a_{A-1} + a_A]/2$, and $a(x) = \min.\{a_i | i < A \text{ and } [a_i + a_{i+1}]/2 \geq x\}$ otherwise. By definition of $t_i(x; c_a)$, $a(x)$ is stepwise increasing in x with at most $A - 1$ jump discontinuities. However, since the max function is continuous, $V^*(c_a, x)$ is continuous in x on $[0, \infty)$ and, save possibly at the points of discontinuity in $a(x)$, differentiable in x on $(0, \infty)$. Specifically, ignoring the points of discontinuity in $a(x)$, substituting for $t_i(x; c_a)$ and differentiating,

$$dV^*(c_a, x)/dx = -\{1 + 2x - 2a_A(1 + x) + a_A^2 - a(x)^2 + 2xa(x)\} < 0.$$

Now, defining $b(x)$ similarly to $a(x)$, we have the difference $W(x; c_a, c_b) \equiv [V^*(c_a, x) - V^*(c_b, x)]$ continuous and almost everywhere differentiable in x. Assuming differentiability at x:

$$dW(x; c_a, c_b)/dx = 2[a_A - b_B] + [a_A - a(x)][2x - (a_A + a(x))] - [b_B - b(x)][2x - (b_B + b(x))]. \quad \textbf{(A-16)}$$

Now suppose $B = 1$. Then $b(x) = b_B \; \forall x$, in which case,

$$dW(x; c_a, c_b)/dx = 2[a_A - b_B] + [a_A - a(x)][2x - (a_A + a(x))].$$

Consider any x such that $a(x) \leq a_{A-1}$. Let $\hat{a}(x)$ denote the smallest elicited action a_i strictly greater than $a(x)$. By definition of $a(x)$,

$$x \leq [a(x) + \hat{a}(x)]/2 \leq [a(x) + a_A]/2. \quad \textbf{(A-17)}$$

Therefore, $[a_A - a(x)][2x - (a_A + a(x))] \leq 0 \; \forall x$. If $[a_A - b_B] \leq 0$, therefore, continuity of $W(\cdot)$ yields $W(\cdot)$ monotonic in x. Suppose $[a_A - b_B] > 0$. Then for all $x \geq [a_A + a_{A-1}]/2$, $dW(x; c_a, c_b)/dx = 2[a_A - b_B] > 0$. So, by $W(\cdot)$ continuous, if $W(\cdot)$ has more than one turning point, there must exist some $x^\circ < [a_A + a_{A-1}]/2$ at which $W(x^\circ; \cdot)$ is a local maximum. Now, if $W(\cdot)$ is differentiable at x°, $a(x)$ is constant on some open neighbourhood about x°, in which case $d^2W(x^\circ; \cdot)/dx^2 = 2[a_A - a(x^\circ)] > 0$: contradiction. Therefore, if $W(x^\circ; \cdot)$ is a local maximum, $W(\cdot)$ cannot be differentiable at x°; in particular, $\exists i = 1, \ldots, A - 2$ such that $a(x - \epsilon) = a_i$ and $a(x + \epsilon) = a_{i+1}$ for $\epsilon > 0$ and arbitrarily small. Moreover,

$$dW(x^\circ - \epsilon;\ \cdot)/dx = 2[a_A - b_B] + [a_A - a_i][2(x - \epsilon) - (a_A + a_i)] > 0$$

and

$$dW(x^\circ + \epsilon;\ \cdot)/dx = 2[a_A - b_B] + [a_A - a_{i+1}][2(x + \epsilon) - (a_A + a_{i+1})] < 0.$$

Hence,

$$[a_A - a_i][2(x - \epsilon) - (a_A + a_i)] > [a_A - a_{i+1}][2(x + \epsilon) - (a_A + a_{i+1})]. \quad \textbf{(A-18)}$$

By A-17, A-18 implies that for all $\epsilon > 0$, ϵ sufficiently small:

$$[a_A - a_i]/[a_A - a_{i+1}] < [2(x + \epsilon) - (a_A + a_{i+1})]/[2(x - \epsilon) - (a_A + a_i)].$$

Since $a_{i+1} > a_i$, $1 <$ LHS(a27). And for ϵ sufficiently small, $(x + \epsilon) \approx (x - \epsilon)$; but then $a_{i+1} > a_i$ implies RHS(a27) < 1: contradiction. Therefore, if $B = 1$ $W(x; \cdot)$ can have at most one turning point. Q.E.D.

Proof of Proposition 3. Suppose $\gamma^*(\cdot)$ is semipooling. By proposition 2, $\gamma^*(\cdot)$ cannot involve any separating segments. To prove γ^* nonmonotonic, therefore, it suffices to show that $\gamma^*(\cdot)$ cannot be either (1) (stepwise) increasing on $[0, \bar{x})$ or (2) (stepwise) decreasing on $[0, \bar{x})$, $\bar{x}$ sufficiently large. Consider these in turn.

CASE 1. Suppose $\gamma^*(x) = c_j\ \forall x \in [x_{j-1}, x_j)$, $j = 1, \ldots, L$, with $x_0 = 0$, $x_L = \bar{x}$, and $c_j < c_{j+1}$ all $j = 1, \ldots, L - 1$. Let $\{a_{1(j)}, \ldots, a_{N(j)}\}$ be the set of actions elicited in the (most influential) lobbying subgame consequent on the group giving a contribution c_j; $a_{i(j)} < a_{i+1(j)}$ all i. Note that $\alpha^*(c_1) = 1$. To see this, suppose not. Then by expression 4, $c_1 = 0$ and $N(1) = 1$; specifically, by Lemma 1.2, $\delta^*(0, \emptyset) = 1/2$. Since $c_2 > 0$, expression 4 implies $\alpha^*(c_2) = 1$ and so $N(2) \geq 2$. By incentive compatibility, $qW(x_1; c_2, 0) = c_2$. By the argument for proposition 1, $a_{1(2)} < 1/2 < a_{N(2)}$. Hence, $W(0; c_2, 0) > W(x_1; c_2, 0)$. But then $qW(0; c_2, 0) > c_2$, contradicting incentive compatibility. Therefore, $\alpha^*(c_j) = 1\ \forall j = 1, \ldots, L$. Now let $m_{i(j)}$ be the message that elicits action $a_{i(j)}$, all i, j, and let $T_j(x) = \{t \in [0, 1] \mid \lambda^*(x, t) = m_{N(j)}\}$. By Lemma 1.1, $x' > x$ implies $T_j(x) \subset T_j(x')$. Therefore, by Lemma 1.2, in a most influential equilibrium, the minimal [resp. maximal] value of $a_{N(j)}$ is achieved if $F(x|c_j)$ puts probability one on x_j [resp. x_{j-1}]. So $a_{N(j)} < a_{N(j-1)}\ \forall j = 2, \ldots, L$. Hence, equation A-16, $dW(x; c_1, c_j)/dx = 2(a_{N(1)} - a_{N(j)}) > 0\ \forall j > 1$ and x sufficiently large. But then $W(\bar{x}; c_1, c_L) > [c_1 - c_L]/q$, contradicting incentive compatibility.

CASE 2. Let $\gamma^*(x) = c_i\ \forall x \in [x_{i-1}, x_i)$, $i = 1, \ldots, L$, $L \geq 2$, and $c_i < c_{i-1}$ all $i > 1$. By previous reasoning, the maximal elicited action in the lobbying subgame is elicited in the subgame consequent on the legislator receiving c_1: $a_{N(1)} > a_{N(j)}$ all $j > 1$. By equation A16, $dW(x; c_1, c_j)/dx = 2(a_{N(1)} - a_{N(j)})$ for sufficiently large $x > x_1$. Hence, for x sufficiently large, $W(x; c_1, c_j) > [c_1 - c_j]/q$, contradicting incentive compatibility.

Now suppose that γ^* is semipooling and that exactly one contribution induces access, say, $\gamma^*(x) = c$. Then sequential rationality requires $c > 0$ and $\gamma^*(y) \neq c$ iff $\gamma^*(y) = 0$. Hence the legislator's unique best response action conditional on $\gamma^*(\cdot) = 0$ is to choose $E[t|\gamma^*(\cdot) = 0, \alpha^*(\gamma^*(\cdot)) = 0] = \int_0^1 tg(t)\,dt = 1/2$. By case 1 and 2 above, γ^* cannot be monotonic. So to prove that it must be (stepwise) U-shaped, it suffices to exclude the possibility that γ^* has any (stepwise) increasing segments followed by (stepwise) decreasing segments on $[0, \bar{x})$. Assume the contrary. Because $\gamma^*(x) \in \{c, 0\}\ \forall x$, this means there exist $0 \leq x_1 < x_2 < x_3 < x_4 \leq \bar{x}$ such that $\gamma^*(x) = 0\ \forall x \in [x_1, x_2)$, $\gamma^*(x) = c > 0\ \forall x \in [x_2, x_3)$, and $\gamma^*(x) = 0\ \forall x \in [x_3, x_4)$. Since $c > 0$, equation 5 implies that at least two actions are elicited in the lobbying subgame consequent upon access. By incentive compatibility, $W(x; c, 0)$ is increasing in x at x_2 and $W(x; c, 0)$ is decreasing in x at x_3. By Lemma 2, $W(x; c, 0)$ is strictly decreasing at $\bar{x}$. Let a_N be the largest action elicited in the lobbying subgame consequent on $\gamma^*(x) = c$. Since $\bar{x} > 1$, $\gamma^*(x) = c$ and Lemma 1 imply $\lambda^*(\bar{x}, t)$ elicits $a_N\ \forall t \in [0, 1]$. Hence, by $W(x; c, 0)$ strictly decreasing at $\bar{x}$, $dW(\bar{x}; c, 0)/dx = 2(a_N - (1/2)) < 0$; but by the argument for proposition 1, $a_N \geq 1/2$: contradiction. Q.E.D.

Notes

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1. See also Drew 1983; Herndon 1982; Langbein 1986; and Schlozman and Tierney 1986, 253—among others.

2. Crawford and Sobel 1982. Informational models of lobbying include Ainsworth 1993; Austen-Smith 1993; Austen-Smith and Wright 1992, 1994; and Potters and Van Winden 1992.

3. See Lohmann 1993 for a formalisation of this argument.

4. The main empirical focus of models adopting the first perspective on access, is on the relationship between a candidate's probability of electoral success, rather than any notion of policy preferences, and the level of contributions he or she can attract. Typically, the prediction is that the relationship is positive, except possibly at very high estimates of electoral success (Baron 1989; Snyder 1990).

5. In Lohmann 1993, there is no such uncertainty and contributions do not signal anything about the value of access. Instead, campaign contributions in her model signal already-acquired, issue-specific technical information. And with this formulation access is irrelevant to any group that gives a positive contribution: since group preferences are common knowledge, all of the relevant information held by such a group is revealed by its equilibrium contribution per se.

6. In earlier versions I claimed that *any* revealing contribution schedule must be (stepwise) U-shaped. However, my supporting argument was faulty. All I can establish at present is the (more important) claim of nonmonotonicity. So while the U-shape can occur (see example 1), it remains an open issue as to whether it always occurs.

7. But Austen-Smith 1993 presents an informational lobbying game of the sort considered here, in which the status quo plays a central strategic role and such complications are considered explicitly. However, there are no campaign contributions and access is assured by assumption. See also Gilligan and Krehbiel 1987.

8. In an earlier version, the group was allowed to choose whether or not to become informed at some cost. However, since the group becomes informed if and only if access is granted, making this decision explicit in the model added little but notation.

9. The example may not report the most influential equilibrium available here. Rather, it is chosen both because it illustrates the principles clearly and because it is relatively straightforward to calculate. (The mass point at zero, for instance, permits easier computation of the legislator's decisions at the lobbying stage but is not essential.) Computational details available on request.

10. It is interesting to note here that unlike in most signaling games, a separating equilibrium is broken by the "good" types (i.e., those most valuable to the legislator) deviating to mimic the relatively "bad" types.

11. For further discussion of this sort of lobbying equilibrium, assuming the group's preferences are known, see, e.g., Crawford and Sobel 1982 and, perhaps more accessible, Gilligan and Krehbiel 1987.

12. Formal details available on request.

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A GRAMMAR OF INSTITUTIONS

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The institutional grammar introduced here is based on a view that institutions are enduring regularities of human action in situations structured by rules, norms, and shared strategies, as well as by the physical world. The rules, norms, and shared strategies are constituted and reconstituted by human interaction in frequently occurring or repetitive situations. The syntax of the grammar identifies components of institutions and sorts them into three types of institutional statements: rules, norms, and shared strategies. We introduce the grammar, outline methods for operationalizing the syntax, apply the syntax to an analysis of cooperation in collective dilemma situations, and discuss the pragmatics of the grammar for analyses of behavior within complex institutional settings.

The core of traditional political science was the study of institutions and of political philosophy. The behavioral revolution swept both aside and focused political scientists on the study of political behavior. Then came the onslaught of rational choice theory. To the surprise of political scientists, these efforts concluded that many fond beliefs about the effects of institutions lacked firm theoretical foundations based on individual choice. The past decade has witnessed a flurry of new scholarship in the rational choice tradition flying the banner of "The New Institutionalism." The flood of work produced in this tradition facilitates a return to the core of the discipline, namely institutions and the nature of political orders.

Renewed interest in institutions has, however, generated a simmering theoretical debate about what institutions are. The debate is healthy and likely to lead to a clarification of core concepts used in political science. The institutional grammar introduced here is based on a view that institutions are enduring regularities of human action in situations structured by rules, norms, and shared strategies, as well as by the physical world. The rules, norms, and shared strategies are constituted and reconstituted by human interaction in frequently occurring or repetitive situations.[1] Where one draws the boundaries of an institution depends on the theoretical question of interest, the time scale posited, and the pragmatics of a research project. One example of an institution encompasses the regularities of action and outcomes in the U.S. Congress. Another limits the focus to only one house or one committee or subcommittee. Likewise, analysis of institutional statements can target broad prescriptions (e.g., Congress may pass legislation regulating interstate commerce) or focus on a narrower prescription (e.g., a cloture rule). We shall introduce a grammar that provides a theoretical structure for analysis of the humanly constituted elements of institutions (i.e., rules, norms, and shared strategies).

INSTITUTIONS

Several approaches have been taken to answer the question, What is an institution? One major approach to this question is the *institutions-as-equilibria* approach drawing on the work of Menger (1963) and von Hayek (1945, 1967) for its intellectual foundations and Schotter (1981), Riker (1980), and Calvert (1992) for contemporary views. Works in this approach focus on the stability that can arise from mutually understood actor preferences and optimizing behavior. Scholars in this tradition treat these stable patterns of behavior as institutions. Lewis (1969), Ullmann-Margalit (1977), and Coleman (1987) articulate an *institutions-as-norms* approach. A third approach, the *institutions-as-rules* approach, draws on Hohfeld (1913) and Commons (1968) for its roots and Shepsle (1975, 1979, 1989), Shepsle and Weingast (1984, 1987), Plott (1986), Oakerson and Parks (1988), North (1986, 1990), E. Ostrom (1986, 1990), V. Ostrom (1980, 1987, 1991, n.d.), Williamson (1985), and Knight (1992) for contemporary developments. The institutions-as-norms and institutions-as-rules approaches both focus on linguistic constraints (spoken, written, or tacitly understood prescriptions or advice) that influence mutually understood actor preferences and optimizing behavior.

All three approaches offer institutional explanations for observed regularities in the patterns of human behavior. The differences among the approaches relate primarily to the grounds on which explanations for observed regularities rest. Viewing institutions as equilibrium behavior rests on an assumption that rational individuals, interacting with other rational individuals, continue to change their planned responses to the actions of others until no improvement can be obtained in their expected outcomes from independent action. To understand why regularized patterns of interaction exist, one needs to ask why all actors would be motivated to produce a particular equilibrium.

There are important philosophical reasons for looking at institutions as equilibria. This view places the responsibility for a social order on the individuals who are part of that order, rather than on some external "state" or "third-party enforcer." It integrates analysis of behavior *within* institutions with analysis of how institutions come into being. By focusing on mutually understood actor expectations, preferences, and optimizing behavior, the analyst can examine institutions that do not require outside enforcement nor irrational commitments to following rules. An institution can be viewed as no more than a regular behavior pattern sustained by mutual expectations about the actions that others will take: "The institution is just an equilibrium" (Calvert 1992, 17).[2] Starting from this position, one avoids the trap of reifying institutions by treating them as things that exist apart from the shared understandings and resulting behavior of participants.

While the institutions-as-equilibria approach offers advantages, disadvantages exist as well. Stable outcomes resulting from shared understandings about the appropriate actions for a particular situation are all treated as if they had similar foundations. Lumping together all shared understandings fails to clarify the difference between shared advice based on prudence, shared obligations based on normative judgements, and shared commitments based on rules created and enforced by a community.

The grounds for looking at institutions as norms rest on an assumption that many observed patterns of interaction are based on the shared perceptions among a group of individuals of proper and improper behavior in particular situations. To understand why some regularized patterns of interaction exist, one needs to go beyond immediate means-ends relationships to analyze the shared beliefs of a group about normative obligations. The grounds for looking at institutions as rules rest on an assumption that many observed patterns of interaction are based on a common understanding that actions inconsistent with those that are proscribed or required are likely to be sanctioned or rendered ineffective if actors with the authority to impose punishment are informed about them. To understand regularized patterns of interaction affected by rules, one needs to examine the actions and outcomes that rules allow, require, or forbid and the mechanisms that exist to enforce those rules.

The three approaches are not mutually exclusive. All start from the individual and build social orders on individualistic and situational foundations. The first approach focuses more on the regularities of outcomes in structured situations. The other two focus on linguistic constraints that influence these regularities. If one slightly refocuses the institutions-as-equilibria approach to one of institutions-as-equilibrium-strategies or, more generally, institutions-as-shared-strategies, the three approaches can be directly compared. Focusing on shared strategies shifts the emphasis away from the outcomes obtained and back to the shared understandings and expectations that influence behavior leading to outcomes. In all three cases, then, constraints and opportunities can be articulated as institutional statements. Each approach, in our view, focuses on a different type of constraint or opportunity.

In light of the merits of each approach, we propose not to argue about whether institutions are rules, norms, or strategies. We use the broad term *institutional statement* to encompass all three concepts. *Institutional statement* refers to a shared linguistic constraint or opportunity that prescribes, permits, or advises actions or outcomes for actors (both individual and corporate).[3] Institutional statements are spoken, written, or tacitly understood in a form intelligible to actors in an empirical setting. In theoretical analyses, institutional statements will often be interpretations or abstractions of empirical constraints and opportunities. We develop a grammar of institutions as a theory that generates structural descriptions of institutional statements.[4] The syntax of this grammar operationalizes the structural descriptions; it identifies common components of institutional statements and establishes the set of components that comprise each type of institutional statement. We shall focus primarily on the syntax of the institutional grammar and only briefly discuss semantics and pragmatics.

Three disclaimers apply. First, we do not argue that the institutional statements explaining behavior are always articulated easily and fully by participants. Knowledge of institutional statements is often habituated and part of the tacit knowledge of a community. Moreover, we do not assume that all individuals recognize the existence of an institutional grammar and explicitly use it to formulate institutional statements. Second, a statement that fits the grammatical structure is not necessarily a meaningful or significant institutional statement.[5] Third, to explain behavior in institutions, one needs to combine the grammar of institutions with a theory of action, just as to understand a language one requires a grammar and a theory of language use (Chomsky 1965, 9). The theory of action animates the structural model of a situation generated by the grammar and by relevant attributes of a physical world (Popper 1967).

The grammar facilitates analysis of the content of institutional statements of distinctions among types of institutional statements, and of the evolution of institutional statements. As with any grammar, its application to existing statements sometimes yields tough judgement calls and counterexamples. The principles that guide the identification of components and the classification of statements cannot eliminate all ambiguity.

THE SYNTAX OF A GRAMMAR OF INSTITUTIONS

The general syntax of the grammar of institutions contains five components: ATTRIBUTES, DEONTIC, AIM, CONDITIONS, and OR ELSE. The five letters (ADICO) provide a shorthand way of referring to the components. Regardless of how institutional statements are expressed in natural language, they can be rewritten in the ADICO format, where:

A ATTRIBUTES is a holder for any value of a participant-level variable that distinguishes to whom the institutional statement applies (e.g., 18 years of age, female, college-educated, 1-year experience, or a specific position, such as employee or supervisor).
D DEONTIC is a holder for the three modal verbs using deontic logic: *may* (permitted), *must* (obliged), and *must not* (forbidden).
I AIM is a holder that describes particular actions or outcomes to which the deontic is assigned.
C CONDITIONS is a holder for those variables which define when, where, how, and to what extent an AIM is permitted, obligatory, or forbidden.
O OR ELSE is a holder for those variables which define the sanctions to be imposed for not following a rule.

All shared strategies can be written as [ATTRIBUTES] [AIM] [CONDITIONS] (AIC); all norms can be written as [ATTRIBUTES] [DEONTIC] [AIM] [CONDITIONS] (ADIC); and all rules can be written as: [ATTRIBUTES] [DEONTIC] [AIM] [CONDITIONS] [OR ELSE] (ADICO). The syntax is cumulative: norms contain all of the components of a shared strategy plus a DEONTIC; rules contain all the components of a norm plus an OR ELSE.[6]

In linguistic terms, the components operate as phrasemarkers and the AIC, ADIC, and ADICO are the basis of shared strategies, norms, and rules respectively (see Chomsky 1965, 17). Linguists focus on the transformations that create spoken sentences from base strings. Institutional analysts will focus more on transforming rules and norms as practiced in ongoing situations to base strings in order to capture the core advice or prescription of the institutional statement in use. The ability to translate existing institutional statements and formal representations of institutional statements into an established format provides many advantages for comparison, analysis, and synthesis that we discuss throughout.

To ease the discussion of the syntax components, we refer throughout to the following examples:

1. All male U.S. citizens, 18 years of age and older, must register with the Selective Service by filling out a form at the U.S. Post Office or else face arrest for evading registration.
2. All senators may move to amend a bill after a bill has been introduced, or else the senator attempting to forbid another senator from taking this action by calling him or her out of order will be called out of order or ignored.
3. All villagers must not let their animals trample the irrigation channels, or else the villager who owns the livestock will be levied a fine.
4. All neighborhood residents must clean their yard when the neighborhood organization organizes a major neighborhood cleanup day.
5. The person who places a phone call calls back when the call gets disconnected.

ATTRIBUTES

All institutional statements for a group apply to a subset of participants from that group. The subset can range from one participant to all participants of the group. A set of ATTRIBUTES establishes the subset of the group affected by a particular statement. If individuals make up the group, the ATTRIBUTES will be individual-level values. Individual-level ATTRIBUTES include values assigned to variables such as age, residence, gender, citizenship, and position. When members of a group governed by a set of institutions are corporate actors, rather than individuals, the ATTRIBUTES are organizational variables (e.g., size of membership, geographic location, or ownership of the residuals). In the first example, the relevant ATTRIBUTES are male, citizen of the United States, and over 18 years old. In the last example, the ATTRIBUTE is the caller who placed the call. The other examples list no specific attribute. When no specific attribute is listed, the default value for the ATTRIBUTES component is all members of the group.[7] This means that the ATTRIBUTES component always has something in it, even when a specific attribute is not contained in the statement. Thus the second example applies to all senators in a legislature, and the third example applies to all villagers in a particular village.

DEONTIC

The DEONTIC component draws on the modal operations used in deontic logic to distinguish prescriptive from nonprescriptive statements (see Hilpinen 1971, 1981; Wright 1951). The complete set of DEONTIC operators (D) consists of permitted (P), obliged (O), and forbidden (F). Institutional statements use the operative phrases *may*, *must*, and *must not* to assign these operators to actions and outcomes. Thus the statement that all members *may* vote assigns the DEONTIC permitted, P, to the action of voting. We represent the assignment of a DEONTIC operator to an action $[a_i]$ as $[D][a_i]$, where D stands for P, O, or F. Similarly, $[D][o_i]$ represents the assignment of a deontic to an outcome.[8]

The three DEONTIC operators are interdefinable (von Wright 1968, 143). If one of the deontics is taken as a primitive, the other two can be defined in terms of that primitive. For example, let us use P as a primitive. $[P][a_i]$ would be read, "One is permitted to do a_i." or "One *may* do a_i." The statement that an act is forbidden $[F][a_i]$ can be restated using P as the primitive as $[\sim P][a_i]$. In other words, when $[a_i]$ is forbidden, one is not permitted to do $[a_i]$. On the other hand, if the negation of an action $[\sim a_i]$ is forbidden, one is obliged to take the action. The statement that an act must be done, $[O][a_i]$, can be defined as $[\sim P][\sim a_i]$. If an action is obligatory, one is not permitted not to do $[a_i]$. Alternatively, we could use F as the primitive. Then $P[a_i]$ can be defined as $[\sim F][a_i]$, and $O[a_i]$ can be defined as $[F][\sim a_i]$. With O

as the primitive, $P[a_i]$ can be defined as $[\sim O][a_i$ or $\sim a_i]$, while $F[a_i]$ can be defined as $[O][\sim a_i]$. Interdefinability also exists for prescriptions that refer to outcomes instead of actions. Any prescription with a DEONTIC assigned to some outcome, o_i, can be restated using either of the other two DEONTIC operators.

The meaning of the deontics obliged (*must*) and forbidden (*must not*) fit well into most conceptions of normative statements. The meaning of permitted (*may*) is more perplexing (Schauer 1991). Susan Shimanoff, for example, concludes that "it is incongruous to talk of rules prescribing behavior which is merely permitted" (Shimanoff 1980, 44). However, statements that assign permission (*P*) to an action are meaningful in at least two ways. In many instances, assigning a *may* to an action is the equivalent of "constituting" that action (Searle 1969). For example, a law stating that an individual may vote in an election is meaningful. It creates an action—voting—that did not exist before. Permission establishes qualifications for positions (i.e., who may vote and who may hold offices). Furthermore, when one speaks of a "right" to take an action or effect an outcome, the person with the right is permitted (rather than required or forbidden) to take that action or effect an outcome. Others, who have a duty to recognize that right, are the ones who are forbidden or required to take actions or effect outcomes.[9]

The interdefinability of deontics plays a key role in unraveling another meaning of permission. Any rule or norm that assigns permission, *P*, can be restated using the logical equivalent $\sim F$. If an action is permitted, then that same action is not forbidden. Those who share a common understanding of that institutional statement recognize that the action is not forbidden. Measures that would be taken to prevent that action—or to react to that action as if it were forbidden—are no longer acceptable. In other words, any institutional statement that assigns *may* or *not forbidden* to an action implies that at least some action that could otherwise be taken by others to prevent or punish the permitted action is forbidden. In the second example, senators must not treat the action of amending a bill as a forbidden action. The OR ELSE indicates the institutional consequence of attempting to treat the permitted action as forbidden (the senator is called out of order or ignored).

The assignment of $\sim F$ operates in a similar manner in institutional statements without an OR ELSE. Consider a legislative body that shares an institutional statement like the following: [All junior members] [*P*] [contest senior members] [in committee hearings]. This is the equivalent to [All junior members] [$\sim F$] [contest senior members] [in committee hearings]. This prescription implies a prescription on senior members not to reprimand or castigate junior members who challenge them in committee hearings. The existence of such a norm does not ensure that all senior members will follow it. However, there will be a shared notion that a rebuke based on seniority alone is inappropriate or unacceptable. If a senior member reprimands a junior member for challenging him or her, then we would expect the junior member to invoke the grant of permission to defend against the senior member's actions. It means something for the junior member to say, "Everyone here knows that I am permitted to challenge senior members in committee hearings."

AIM

The AIM is *the specific action or outcome to which an institutional statement refers.* Anything thought of as an AIM must be physically possible (von Wright 1963). An individual cannot logically be required to undertake a physically impossible action or effect a physically impossible outcome. Further, in order for any action to be conceived of as an AIM, its negation $[\sim a_i$ or $o_i]$ must also be physically possible. In other words, including an action or outcome in the AIM component implies that it is avoidable. Thus the capability of voting implies the capability of not voting. Voting for candidate A implies the option of not voting for candidate A.

In the first example given earlier, the AIM is the action of registering for the Selective Service and the DEONTIC operator required, *O*, is assigned to the action for all individuals with the ATTRIBUTES listed in the rule. In the second example, the AIM is the action of offering a motion to amend a bill, the DEONTIC operator is *P*, or permitted for all senators. The AIM in the fourth example also includes an action. The norm requires the action of cleaning up the yard. The third example assigns the DEONTIC *F* to the outcome of livestock damage. The AIM of the rule does not specify actions that an irrigator must, may, or may not take. The AIM only specifies the forbidden outcome. Villagers may select any actions that are not forbidden by another rule to keep their livestock from damaging the irrigation channel. In the fifth example, the AIM is the action of calling back.

CONDITIONS

CONDITIONS indicate the set of variables that define when, where, and how an institutional statement applies. The CONDITIONS for a statement might indicate when a statement applies, such as during certain weather conditions or at a particular stage in the legislative process. Likewise, the CONDITIONS might indicate where a statement applies (e.g., within a particular jurisdictional area) or how a statement is to be followed (e.g., through a defined process). If an institutional statement does not specify a particular condition, the default value for the condition is "at all times and in all places." Thus, like the ATTRIBUTES, the CONDITIONS component always has some value, regardless of whether an institutional statement overtly specifies conditions.

The CONDITIONS component in the first example states how the statement is to be followed, namely, by filling out a form at the U.S. Post Office. It does not restrict when or where the statement applies. The

CONDITIONS component in the second and the fourth example indicate when the prescription applies. After a bill has been introduced, the prescription of the second rule applies. Any senator may move to amend a bill after it has been introduced. The fourth example applies on the days designated as cleanup days by a neighborhood organization, and the fifth example applies when a phone call is disconnected. The third example does not specify any specific CONDITIONS. We assume that the rule applies for members of the village at all times and in all places with no additional restrictions on how the rule is to be followed.

OR ELSE

The final component of our institutional syntax, the OR ELSE, is the sanction assigned to detected noncompliance with an institutional statement. Only rules include this component. The content of the OR ELSE affects the very nature of a rule. Rowe (1989) discusses the difference between a speed-limit law with minor sanctions and a speed-limit law with a severe penalty as the sanction. The prescription is the same. For both laws, the severity of the sanction in the OR ELSE is the only difference, but expected behavior is different.[10]

In order for threats associated with prescriptions to qualify as an OR ELSE, they must meet three qualifications. First, the threat must be backed by another rule or norm that changes the DEONTIC assigned to some AIM, for at least one actor, under the CONDITIONS that an individual fails to follow the rule to which the threat applies. The rule or norm backing an OR ELSE establishes both a range of punishments available and assigns the authority and procedures for imposing an OR ELSE. Often the actions threatened in the OR ELSE are forbidden under most CONDITIONS (e.g., imposing a fine, incarcerating a citizen, or putting someone's livestock in a village pen). The prescription backing the OR ELSE makes these actions permitted in the CONDITION that someone breaks a rule. However, the shift in the DEONTIC is not always from *F* to *P* or *O*. The OR ELSE might involve forbidding some action that is usually permitted or obligatory, a shift from *P* or *O* to *F*. For example, a license branch official could be forbidden to issue a drivers license to a citizen after that citizen has been convicted of repeated drunken driving offenses. Although the OR ELSE often refers to physical punishments, the OR ELSE may also refer to institutional actions such as taking away a position or refusing to accept an amendment as legal. For example, one of the rules governing the amendment process could state that legislators with [ATTRIBUTES] [must] [obtain positive votes from at least a majority] [when voting for an amendment] or [the amendment fails].

Second, institutional statements must exist that affect the constraints and opportunities facing actors who monitor conformance to the prescription. Monitoring institutions may assign these tasks to specialized individuals or authorize all participants to engage in these activities. Although the actors who monitor also frequently sanction nonconforming actors, they may only report nonconformance to someone else who is responsible for sanctioning.

Third, the OR ELSE must be crafted in an arena used for discussing, prescribing, and arranging for the enforcement of rules. This arena may be a legislature or a court, but we do not consider government sponsorship or government backing to be a necessary condition for an institutional statement to be a rule. Many self-organized, communal, or private organizations develop their own rules that include (1) sanctions backed by another rule or norm that change the DEONTIC assigned to some AIM for at least one actor under the condition that individuals fail to follow the rule and (2) institutional statements that affect the constraints and opportunities of actors to take the responsibility to monitor the conformance of others to the prescription.[11] For some research questions, it may be useful to divide rules into those made within governmental arenas and those made outside of government arenas. The syntax in no way prevents this distinction.

The first four examples have the syntax of a rule, if we assume that all three qualifications for the OR ELSES in these statements are met. For example, the potential punishment for villagers who let their livestock trample the irrigation channels qualifies as an OR ELSE only when a second rule or norm accepted in that village prescribes others to employ the sanctions defined in the OR ELSE, an institutional statement prescribes or advises monitoring, and these arrangements were made in a setting considered by the villagers to be a rule-making arena.

OPERATIONALIZING THE SYNTAX

All linguistic statements that explain shared strategies contain three of the syntax components (AIC), those that explain norms contain four (ADIC), and those that explain rules contain all five (ADICO). Parsing the five examples using *F* as the primitive DEONTIC yields the following:

1. U.S. citizens [age ≥ 18, gender = male] [*F*] [~(register with Selective Service)] [filling out form at U.S. Post Office] [face arrest].
2. Senators [all] [~*F*] [move an amendment] [after bill introduced] [called out of order or ignored].
3. Villagers [all] [~*F*] [irrigation channel trampled by their animals] [at all times] [fine].
4. Neighborhood residents [all] [*F*] [~(clean yard)] [organized cleanup day] [ ].
5. Person [placed call] [ ] [calls back] [call disconnected] [ ].

Now, how can the syntax be operationalized in empirical or theoretic research? In empirical studies, the researcher's task is to discover the linguistic statements that form the institutional basis for shared expectations that influence observed regularities in behavior. Essentially, this entails discovering which ADICO components exist in these statements and the contents of those components. Determining the existence of a component requires knowledge about whether individuals in a group share some level of common understanding of its content. In the case of an OR ELSE, complementary institutional statements are needed, as well as evidence that individuals crafted the OR ELSE in a rule-making arena. Uncovering components requires qualitative research methods, including in-depth interviews or archival retrieval. Using the syntax to structure game-theoretic analysis requires the researcher to identify how the components of the institutional statements assumed in the game are reflected in the structure of a game and in the payoffs assigned to actions and outcomes.

Operationalizing the ATTRIBUTES, AIM, and CONDITIONS

The AIM usually supplies the focus for theoretical and empirical studies. Scholars decide to study the impact of institutions on behavior for some subset of actions or outcomes. Studies of agenda setting and voting institutions, for example, focus on institutional statements with AIMS that relate to setting agendas and voting. Once researchers select the focal set of actions or outcomes for their studies, the next step is developing the analysis of institutional statements with AIMS that relate to those actions or outcomes.

The existence of the ATTRIBUTES and CONDITIONS components is fairly straightforward; they always exist if an institutional statement exists. In some cases they exist in their most general form—all individuals or all organizations and under all conditions.[12] Discovering the contents of the ATTRIBUTES and CONDITIONS components requires the empirical researcher to inquire about the actors to whom particular institutional statements apply and the conditions under which they apply. The syntax reminds formal theorists that there are assumptions about ATTRIBUTES and CONDITIONS built into the games that they model. Game-theoretical models that are constructed to provide an institutional explanation for observed regularities of behavior make explicit assumptions about the ATTRIBUTES and the CONDITIONS included in the institutional statements of interest. Recognizing the implicit existence of these components helps scholars to discover and explicitly state those assumptions.

Operationalizing the DEONTIC

The first step in operationalizing the DEONTIC is deciding whether the institutional statement contains one. In empirical studies, the researcher listens for normative discourse. Is there an articulated sense of moral or social obligation expressed? If the individuals in a study share only AIC statements, then their discussion of why they would follow such advice focuses only on prudence or wise judgement: "The best thing to do when faced with a choice between A and B under condition Y is to choose A, because one is usually better off with this choice." When individuals shift to a language of obligation, they use terms such as *must, must not, should,* or *should not* to describe appropriate behavior (Orbell, van de Kragt, and Dawes 1991): "The obligatory action when faced with a choice between A and B under condition Y is to choose A, because this is the proper action." In other words, one looks for institutional statements that are used to evaluate behavior (Collett 1977). When social and moral obligations are discussed, an empirical researcher initially assumes that it is appropriate to include a DEONTIC in institutional statements used to explain behavior.

When a DEONTIC exists, one expects negative repercussions to follow behaviors that do not conform to the prescription and positive rewards to follow compliance. We use a delta parameter concept to capture the costs or benefits of these inducements and deterrents for any particular DEONTIC. Usually, we expect the severity of the delta parameters to provide some indication of the importance or valence of the DEONTIC to the community.[13]

Delta parameters may reflect internal and/or external effects of a DEONTIC. The distinction between external and internal sources of delta parameters is similar to the one that Coleman (1987) makes between "internalized norms" and "externally sanctioned norms" and that Kerr and his colleagues (1994) make between a "personal" and a "social" norm. Delta parameters originating from external sources are a way to represent the benefits and costs of establishing a reputation (see Kreps 1990). The delta parameters originating from internal sources can be thought of as the guilt or shame felt when breaking a prescription and the pride or "warm glow" felt when following a prescription, particularly if it is costly to follow in a particular situation (Andreoni 1989; Frank 1988; Ledyard 1995).

If an action is forbidden by a norm and an individual engages in that action, then we expect that person to experience some type of cost represented by at least one delta parameter. If norms indicate that an AIM is *permitted,* then we expect others, who treat that action as if it were forbidden, to experience some cost. In other words, norms or rules that forbid or require some AIM will be reflected in a cost parameter to the individuals to whom the prescription applies, while prescriptions that establish permission for an action place the cost parameter on others. Others may experience a cost if they try to obstruct an individual when a shared norm indicates that the individual is permitted to take that action.[14]

Using delta parameters brings normative considerations overtly into the analysis of action and consequences, rather than employing ad hoc interpretations. Adding overt consideration of nonmaterial costs and benefits is not the same as assuming that individuals incorporate the welfare or utility of other actors into their own calculus; nor is it the same as substituting the collective benefits for individual utility. We prefer the delta parameter interpretation of "other" or "normative" motivation because it ties that motivation to institutions by representing individual commitments to the norms or rules of a situation. Our preference for the delta parameter interpretation is bolstered by recent evidence that group discussion affects cooperation in social dilemma experiments in a manner that supports the logic of a delta effect and disputes a group identification effect (Kerr n.d.).

In field research, the deltas are frequently not observable, especially in situations where behavior prescribed by a norm is habituated. First, one may not observe any instances of nonconformance so that no opportunity exists to witness reactions to deviations. Second, few external rewards for conformance occur in situations where everyone expects conformance. In an empirical setting where one cannot observe repercussions or see tangible rewards, one has to rely primarily on the accounts that participants give as to the normative content of their actions and the repercussions of nonconformance.

In formal analyses, one can model the DEONTIC by adding delta parameters to the players' payoffs to represent the perceived costs and rewards of obeying (δ^o) or breaking (δ^b) a prescription. The delta parameters can be defined as:

$\Delta = \delta^o + \delta^b$, where

Δ = the sum of all delta parameters

δ^o = the change in expected payoffs from obeying a prescription

δ^b = the change in expected payoffs from breaking a prescription.

One can further divide these rewards and costs into those that arise from external versus strictly internal sources of valuation. Thus:

$\delta^o = \delta^{oe} + \delta^{oi}$ and $\delta^b = \delta^{be} + \delta^{bi}$, where

e = the change in expected payoffs originating from external sources

i = the change in expected payoffs originating from internal sources.

The analyst may not wish to focus on all four parameters in any particular analysis. Three of the four delta parameters could be assigned a zero value in a game-theoretic analysis involving a norm or a rule. In order to analyze the impact of a DEONTIC on expected outcomes of a game, however, at least one of the delta parameters must have a nonzero value. The payoff structures for individuals who share prescriptions must differ from those of players in a similar situation in which players merely accept a shared understanding of prudent action.

In situations where it is reasonable to assume that all players who break the prescription feel the same cost, the delta parameters can be modelled as if they were the same for all players and as if their magnitude is public information. Alternatively, the theorist can model players as being different types who react differentially to breaking prescriptions (see Harsanyi 1967–68). One type of player can perceive the costs of breaking a prescription (δ^{bi} or δ^{be}) to be high while another type perceives costs (δ^{bi} or δ^{be}) to be low. Coleman's (1988) zealot, for example, is a player with high external deltas for obeying norms (a high positive δ^{oe}).

The existence of a DEONTIC implies the presence of additional information that individuals use in developing their expectations about others' behavior and thus their own best response. If players all adopt a norm, the payoff structure differs from the payoff structure for a similar situation in which the players do not adopt a norm. The payoffs may even change enough so that the predicted outcome of the game changes. Uncertainty about the presence of actors who have accepted certain norms may be sufficient grounds for changing the behavior of other players. Kreps and his colleagues (1982) have analyzed repeated prisoner's dilemma games where information asymmetries exist among players concerning the probability that other players will play Tit-for-Tat.[15] In such games, players who are "perfectly rational" (i.e., the players' payoff functions have a zero value for all four delta components associated with playing Tit-for-Tat) will adopt behavior consistent with the norm for most of the game, due to their changed expectations about the behavior of others.

Operationalizing the Concept of OR ELSE

The criteria for indicating that an OR ELSE exists are (1) a known range of sanctions, (2) a norm or rule that prescribes sanctions, and (3) some provision for monitoring—all emanating from a rule-making arena. We wish to emphasize that the sanction in an OR ELSE differs from a logical consequence or an interpersonal threat or retaliation. Neither of these repercussions fit the OR ELSE criteria. In empirical analyses, researchers determine whether observed regularities in behavior are based on a shared understanding about the sanctions associated with a rule and whether there is a shared norm or rule that changes the DEONTIC associated with the sanctioning act for some player that can be traced to a rule-making arena. If so, explanations of behavior would consider the attitudes or feelings about following rules (the value of internal delta parameters), the reputation effects of compliance or noncompliance (the external delta parameters), the chance of being observed, the probability that the OR ELSE sanction would be imposed, and the size of the OR ELSE sanction.

In the ADICO syntax, a prescription used to explain the behavior that fails to meet the OR ELSE test is a norm. In other words, a written law could be a norm rather than a rule if the law does not meet the OR ELSE requirements. Analysis of the decisions that individuals make to follow or break such a norm (written as a law) would be based primarily on their own feelings about conformance (δ^{oi} and δ^{bi}) and their appraisal of potential reactions from others (δ^{oe} and δ^{be}), their estimates of the values that other players place on conformance (the other players' δ^{oi} and δ^{bi}), and their estimates of the values that other players place on feedback (the other players' δ^{oe} and δ^{be}).

To incorporate the syntax into formal analysis of rules and behavior, the payoffs for actions governed by rules need to include delta parameters that reflect the DEONTIC and a parameter representing the sanction defined in the OR ELSE. If the enforcing players are brought into the analysis, the enforcing players would have delta parameters in their payoffs since there is a rule or norm that prescribes sanctioning. If the OR ELSE is backed by rule, then we expect the payoffs for sanctioning or not sanctioning to include delta parameters and a variable representing the punishment defined in the OR ELSE of the sanctioning rule. If it is costly to monitor the actions of others and/or to impose sanctions on them, those assigned these tasks may not be motivated to undertake these assignments unless (1) the monitor and sanctioner face a probability of an OR ELSE, (2) social pressure to monitor and sanction is large and is salient to the monitor and sanctioner (large δ^{oe} or δ^{be}), (3) the monitor and sanctioner hold some strong moral commitment to their responsibilities (large δ^{oi} or δ^{bi}), and/or (4) the payment schemes for the monitor and sanctioner create prudent rewards high enough to offset the costs. When a norm backs an OR ELSE, enforcement rests solely on the value of the delta parameters and on the payment scheme for the monitor and sanctioner.

USING THE SYNTAX FOR SYNTHESIS

In any science, understanding what others have already discovered is an important part of research. Synthesizing findings from the different subfields that relate to each type of institutional statement is an important task for those interested in institutions. The similarities between rules, norms, and shared strategies in the ADICO syntax illustrate why the literature so frequently mixes the concepts or uses them interchangeably. They do share several of the same features. Table 1 sorts the concepts that other authors use into the types of institutional statements created by our syntax. In other words, all of the terms used by other authors, shown in the top section of the table, appear to describe institutional statements that are shared strategies according to the syntax; they contain AIC components. The need for a consensus in the use of terms is vividly illustrated by examining the number of different terms in each section of the table. Moreover, several terms appear in all three sections. Notice that some of the terms used by an author appear in more than one of the categories. This means that the term could fit either of the categories in the syntax given the diverse ways that the author uses the term in the work cited. That these terms have been used in so many different ways is not a criticism of past work. Rather, it portrays the difficulty of untangling these concepts so that a less ambiguous set of definitions can be used as the foundation for further theoretical work.

TABLE 1

Shared Strategies, Norms, and Rules as Used in ADICO Syntax and in Recent Literature

TERMS	CITATIONS
AIC	
Shared strategies	*The Present Article*
Norms	Axelrod 1986
Rules, strategies	Axelrod 1981
Doxic elements of action	Bourdieu 1977
Equilibrium strategies	Calvert 1992
Norms	Levi 1990
Rules	March and Olsen 1989
Taken-for-granted actions	Meyer and Rowan 1991
Rules	Myerson 1991
Rules of action	Rowe 1989
Scripts	Schank and Abelson 1977
Focal points	Schelling 1978
Institutions	Schotter 1981
Conventions	Ullmann-Margalit 1977
ADIC	
Norms	*The Present Article*
Conventions	Braybrooke 1987
Rules	Braybrooke 1994
Norms	Coleman 1987
Institutions	DiMaggio and Powell 1991
Conventions	Lewis 1969
Norms	Levi 1990
Rules	March and Olsen 1989
Taken-for-granted actions	Meyer and Rowan 1991
Ethical codes	North 1981
Obligations	Rowe 1989
Institutions	Schotter 1981
Conventions	Sugden 1986
Social norms	Ullmann-Margalit 1977
Conventions	Weber 1947
ADICO	
Rules	*The Present Article*
Norms backed by metanorms	Axelrod 1986
Working rules	Commons 1968
Norms	Coleman 1987
Legalistic institutions	Levi 1990
Rules	Knight 1992
Rules	North 1990
Rules	Shepsle 1979, 1989
PD norms, decrees	Ullmann-Margalit 1977
Laws	Weber 1947
Rules of the game-form	Hurwicz 1994

A growing body of work considers the mix of normative and material motivations that individuals consider when faced with choices (Coleman 1988; Ellickson 1991; Elster 1989a, 1989b; Etzioni 1988; Hirschman 1985; Knack 1992; Mansbridge 1990, 1994; Margolis 1991; Offe and Wiesenthal 1980; E. Ostrom 1990; V. Ostrom 1986; Udéhn 1993). These works treat the normative aspects of decisions up front as a significant part of the analysis. Margolis argues for the necessity of such an approach: "If we analyze everything in terms of strict self-interest and then

TABLE 2

Delta Parameters and Normative Concepts Used in Recent Literature

DELTA PARAMETERS	CONCEPTS USED BY OTHER AUTHORS	CITATIONS
Size, sign, and interpretation of delta parameters		
$+\delta^{oi}$	warm glow	Andreoni 1989; Ledyard 1995
$+\delta^{oe}$	encouragement	Coleman 1988
	status improvement/ reputation enhancement	Coleman 1988
	honor	Ullmann-Margalit 1977
$-\delta^{bi}$	duty	Knack 1992
$-\delta^{be}$	cost of being punished (P & P′)	Axelrod 1986[a]
	social sanctions	Knack 1992
	third-party sanctions	Bendor and Mookherjee 1990[b]
$+\delta^{oi}$ and $-\delta^{bi}$	internalized norms	Coleman 1987
	public spiritedness	Mansbridge 1994
	moral duty	Etzioni 1988
	duty	Commons 1968
$+\delta^{oe}$ and $-\delta^{be}$	externally sanctioned norms	Coleman 1987
	reputation	Kreps 1990
	responsibility	Commons 1968
	moral judgment	Sugden 1986
Types of players		
$+\delta^{oe}$ large	zealot	Coleman 1988
$\Delta = 0$	selfish rational individual	Elster 1989a
$+\delta^{oi}$ and/or $-\delta^{bi}$ large	Everyday Kantian	Elster 1989a
$+\delta^{o}$ large when # of cooperators low	Elite participationists	Elster 1989a
$+\delta^{o}$ large when # of cooperators high	Mass participationists	Elster 1989a
Δ larger when # of cooperators > threshold	People motivated by fairness	Elster 1989a
Creation and maintenance of delta parameters		
Δ affected by labor union activities		Offe and Wiesenthal 1980
Δ are scarce resources that erode with use		Olson 1991
Δ are resources that increase with use		Hirschman 1985; Mansbridge 1994
Δ affected by external fines		Frey 1994
Δ lower when rules come from outside authority		Frey 1994

[a] In some cases, these sanctions may meet the criteria of an OR ELSE.
[b] As with the P and P′ of Axelrod's, the third-party sanctions may at times meet the criteria of an OR ELSE.

include some social motivation only if we get stuck or if there is something left over, it is not likely to lead to nearly as powerful a social theory as if the two things are built in at the base of the analysis" (1991, 130). Delta parameters provide a conceptual language with which to build normative considerations into analysis from the beginning and to discuss differences in studies that incorporate normative incentives.

Table 2 lists studies that have addressed three types of questions about normative motivations. The top section of the table lists different assumptions regarding the meaning and sign of delta parameters. Knack's (1992) analysis of voter turnout, for example, illustrates the insight possible from a careful study of internal and external normative influences. He offers empirical evidence of the substantive content of internal and external pressures associated with a turnout norm and of the influence of these pressures on the probability that an individual will vote. Interestingly, his interpretation of the declines in voter turnout echoes the importance of monitoring. He finds that social sanctions (external deltas) are a key influence on voting turnout. Social pressure only operates, however, when voters expect to be in situations where someone will ask them if they voted. As the percentage of individuals in organizations, in relationships with neighborhood residents, in extended family situations, and in marriages decreases, this monitoring decreases and the power of the social sanction (an external delta) diminishes.

The middle section of Table 2 cites work that addresses the implications of various mixes of different types of individuals or players. For example, a selfish rational individual would be a type that assigns a zero value to praise or blame for obeying or breaking prescriptions. One interesting variant of types ties the size of the delta to the number of others who conform to the prescription; its size is conditional. Elster (1989a) examines the consequences of communities with various mixes of types with conditional and constant deltas.

Scholars cited in the bottom section of Table 2 discuss variables that influence the direction and size of delta parameters. Offe and Wiesenthal (1980) examine the costs that labor unions face to build and maintain a shared commitment to participation norms as this in turn affects their ability to compete with other interest groups. Several other authors ask whether normative incentives increase or decrease with use. Olson (1991) views the delta parameters as scarce resources that can be dissipated with too much use, while Hirschman (1985) and Mansbridge (1994) come to the opposite conclusion. They argue that the normative constraints increase in size as they are used repeatedly by individuals in a group. Frey (1994) contends that external interventions, such as fines, adversely affect the size of delta parameters, particularly the internal deltas. He also speculates that the deltas associated with rules will be higher when individuals participate in making their own rules than when rules are made by higher authorities.

USING THE GRAMMAR FOR ANALYSIS

Now our applications of the grammar move from synthesis to analysis of collective-action problems. The issue of how individuals solve collective-action problems is at the forefront of attention across the social sciences. The disjunction between theoretical predictions of complete free-riding in Prisoner's Dilemma situations and the rates of cooperation that case studies and laboratory experiments reveal provokes much discussion (Udéhn 1993; Weiss 1991). The scholarly discourse about these issues involves terms such as common understandings, shared beliefs, scripts, norms, rules, procedures, institutions, informal rules, informal institutions, conventions, internal solutions, external solutions, as well as a wide diversity of highly technical terms related to particular solution theories.

To illustrate how the ADICO syntax clarifies such analysis, we model the effect on behavior of three structural adjustments (represented by additions of an ADICO component) to a social dilemma situation. This application of the grammar employs game theory and stresses formal analysis. We use existing game theory as our illustrative theory of action. However, the logic discussed in the application applies to nonmathematical analyses as well.

Collective-action problems can be represented by many different game structures (see Taylor 1987). However, because most social scientists know the Prisoner's Dilemma game well, we use a Prisoner's Dilemma game so that we can more easily jump into existing debates and rely on extensive prior work. We start with a simple two-person Prisoner's Dilemma game and use the ADICO format to illustrate the research issues, the game structures, and the predicted outcomes that arise from Prisoner's Dilemma situations when we add: (1) shared expectations of other players' behavior only (AIC statements), (2) normative views of the appropriate actions to be taken (ADIC statements), and (3) rules (ADICO statements). Table 3 summarizes the institutional and payoff characteristics of four games based on a two-person Prisoner's Dilemma situation.[16] The first game is the base two-person Prisoner's Dilemma game. The *shared strategies* game adds a set of shared strategies that equate to the *grim trigger* strategy. The *norms* game adds a cooperating norm to the base situation. The *rules* game adds a cooperating rule, a monitoring norm, and a sanctioning norm to the base Prisoner's Dilemma game. These four examples represent only one way to add the ADICO statements to a two-person Prisoner's Dilemma game.

The Base Game

In the two-person Prisoner's Dilemma base game, we assume that $1 > c > d > 0$. The best payoff (1) comes from choosing *D* while the other player chooses *C*; the second best payoff (*c*) occurs for both players choosing C; third is the payoff (*d*) resulting from both players choosing *D*; and the worst payoff (0) comes from choosing *C* while the other player chooses *D*. Given these payoffs, both players are better off choosing *D* no matter what the other player chooses. The solution to this game, if played only once, is for both players to choose *D* and receive *d*, instead of the more desirable c that they could have received if they had both chosen C. Even if repeated a finite number of times, the solution is for both players to always choose *D*.

The Shared Strategies Game

Predictions that individuals will select *C* rather than *D* in a Prisoner's Dilemma game based on shared strategies rely upon changes in players' expectations about each other's future behavior. In order to incorporate those expectations into formal analysis, we use an indefinitely repeated version of the base game in which future expected payoffs are part of a player's calculation at any one round. In the shared strategies game in Table 3, the players all use a grim trigger: all players cooperate in each round of the game until someone defects, after which players defect for the rest of the game.[17] Assuming that players do not discount future payoffs, both players cooperating in every round is predicted if and only if the expected payoffs from cooperating are greater than the expected payoffs for defecting: $c + t(c) > 1 + t(d)$. This simplifies to

$$c > \frac{1 + d}{1 + t},$$

TABLE 3

Game Summaries

INSTITUTIONAL STATEMENTS	PAYOFFS
Base Game NONE (physical world)	*Player 1 or 2* C = c if other C = 0 if other D D = 1 if other C = d if other D
Shared Strategies Game AIC statements [All players] [ ] [C] [first round] [ ] [All players] [ ] [C] [if all C in previous round] [ ] [All players] [ ] [D] [all rounds after a D] [ ]	*Players 1 or 2* C = c + t(c) if other C = 0 + t(d) if other D D = 1 + t(d) if other C = d + t(d) if other D
Norms Game ADIC statement [P1[a] & P2] [must] [C] [always] [ ]	*Players 1 or 2* C = base game payoffs + δ^{oi} + δ^{oe} if P3 → M[b] = base game payoffs + δ^{oi} if P3 → ~M D = base game payoffs + δ^{bi} + δ^{be} if P3 → M = base game payoffs + δ^{bi} if P3 → ~M *Player 3* M = E if (P1 & P2)→ C = R–E if (P1 or P2) → D ~M = 0
Rules Game ADICO statement [P1 & P2] [must] [C] [always] [*f*]	*Players 1 and 2* C = norm game payoffs D = norm game payoffs + *f* if (P3 → M) & (P4 → S) = norm game payoffs if (P3 → ~M) or (P4 → ~S)
ADIC statements [P3] [must] [monitor] [always] [ ] [P4] [must] [impose *f* on defector] [when P3 reports a D] [ ]	*Player 3* M = norm game payoffs + δ_m^o ~M = norm game payoffs − δ_m^b *Player 4* Only plays if P3 → M S = $\delta_s^o - E_s$ ~S = $-\delta_s^b$

[a] P1 refers to player 1 and so on.

[b] (P3 → M) indicates that player 3 chooses M.

where t is the expected number of future rounds.[18]

All players must share common knowledge that all have adopted a trigger in order for it to be an institutional statement. If all players do not consider it prudent to defect for all rounds after someone initially defects, the trigger strategy is not shared and will not work. Little empirical evidence exists that individuals share a belief in the prudence of a grim trigger (Ostrom, Gardner, and Walker 1994). Herein lies the frailty of the grim trigger as either a resolution of the Prisoner's Dilemma game or a possible institution.

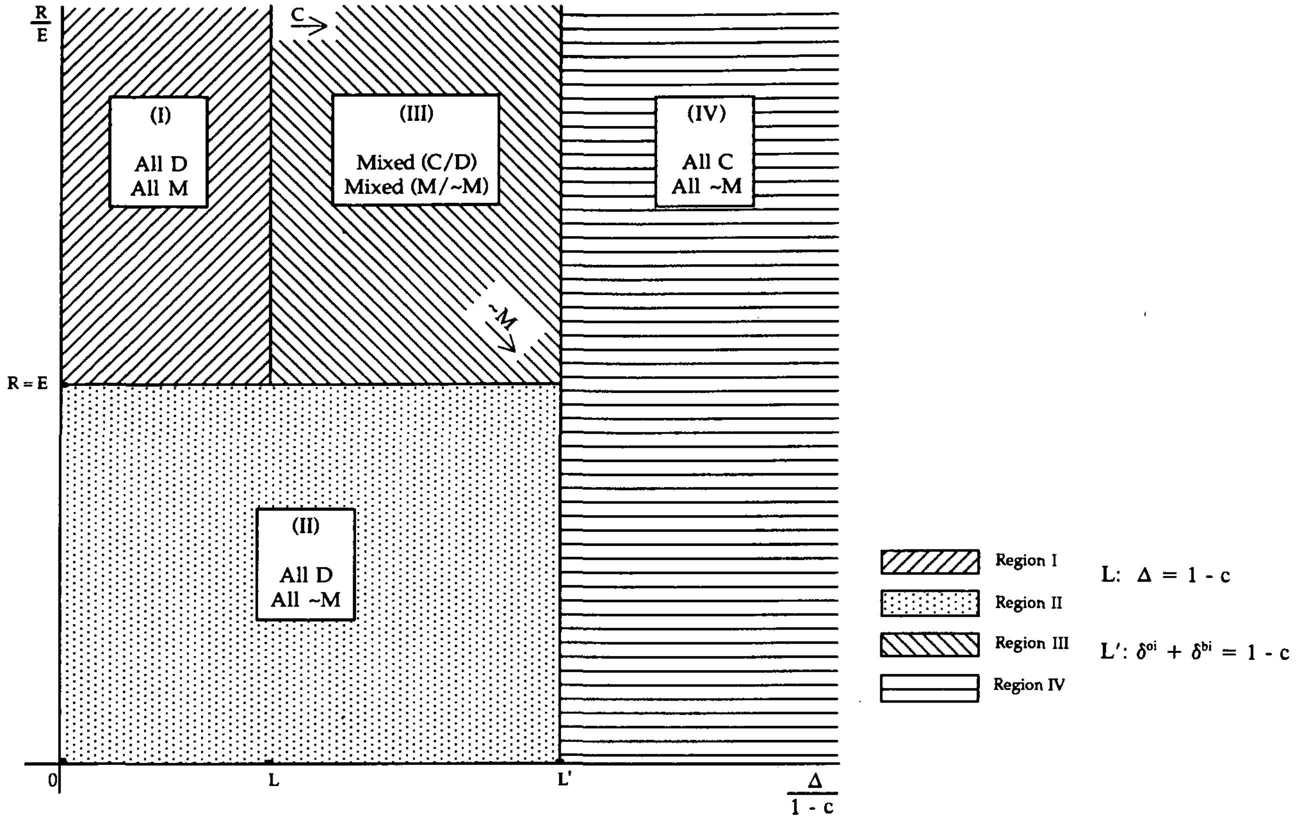
The Norms Game

Predictions that individuals will select C rather than *D* in a Prisoner's Dilemma base game based on norms rely upon changes in players' payoffs because of the addition of at least one delta parameter. To avoid the case of predicting cooperation as a dominant strategy through only internal commitments to norms, we add both external and internal delta parameters. We allow nonzero internal and external delta parameters for both obeying the cooperation norm (δ^{oi} and δ^{oe}) and for breaking the cooperation norm (δ^{bi} and δ^{be}).

In order to make the discussion more applicable to situations with more than two players, we add a third player, a *monitor,* who chooses to monitor (M) or not to monitor ($\sim M$). We assume that external reinforcements for obeying (δ^{oe}) or for breaking (δ^{be}) a norm only occur when the monitor detects the choices of a player. In the simple two-person game, this assumption is not necessary. Players 1 and 2 know whether the other player cooperated by simply looking at their own payoffs. However, as soon as the number of players in a Prisoner's Dilemma is larger than 2,

FIGURE 1

Equilibrium Diagram: Game with a Norm and Monitoring



identifying who cooperates and who defects is no longer trivial.[19] The monitor in this game is motivated solely by prudential rewards associated with discovering defection.[20] There is no monitoring norm or rule.

In the norms game, predictions about players' strategies depend on the relationships among the original payoffs in the base game, the added delta parameters, and the benefits that the monitor receives for reporting nonconformance. This game has many equilibria. Assuming that all of the delta parameters are symmetric (player 1 and player 2 have the same values for each delta parameter) and that the sum of the external parameters is greater than the sum of the internal parameters (i.e., the social pressure to follow the prescription is greater than the internal pressure), four equilibrium regions exist as shown in Figure 1.

The vertical axis is the ratio of the monitor's reward for detecting defection to the expense of monitoring (R/E). The vertical axis is divided into regions above and below the point at which the reward to the monitor equals the expense ($R = E$). The reward is higher than the cost above this point and lower than the cost below this point.

The horizontal axis is the ratio of the sum of all delta parameters to the advantage of choosing D [$\Delta/(1 - c)$]. Since we assume that players 1 and 2 have the same values for each of the delta parameters and that both internal and external delta parameters increase monotonically from left to right, a single axis represents the relationship between the sum of all the delta parameters and the advantage of defecting. As one moves to the right, the size of the delta parameters relative to the advantage of D increases. At point L, the sum of the delta parameters equals the advantage of defection when the other player cooperates ($\Delta = 1 - c$). Point L' on the horizontal axis represents that point where the portion of the sum of delta parameters derived from the internal deltas equals the advantage of defection ($\delta^{oi} + \delta^{bi} = 1 - c$). Thus the area to the right of L' represents the region where the internal deltas offset the advantage of defecting and the value of the external deltas becomes irrelevant. In this area (equilibrium region IV on Figure 1), both players choose C. This is the trivial case in which C is the dominant strategy because the internal costs and rewards for players 1 and 2 are sufficiently high to offset the fact that 1 is greater than c.

In region III, both players 1 and 2 select a mixed strategy between C and D.[21] As one moves from left to right in region III, the sum of the delta parameters increases and thus the probability that players 1 and 2 assign to selecting C increases. In this region, the monitor also selects a mixed strategy. The *relative amount* of the monitor's reward decreases as one moves from north to south. Since the monitor receives a reward only if defection is detected, the *probability* of obtaining a reward decreases as one moves from west to east because the probability of defection decreases. The combined effect is that the monitor has the least incentive to monitor when relative rewards are low and the probability of defection is low (in the southeast corner) and the greatest incentive to monitor when the relative rewards are high and the probability of defection is high (in the northwest corner of this region). Thus, as one moves from northwest to southeast in this region, the probability assigned to $\sim M$ increases.

In equilibrium regions I and II of Figure 1, D remains the dominant strategy for players 1 and 2 as it is in the base game, but for different reasons. In region II, the expenses of monitoring are higher than the expected rewards to the monitor. Thus the monitor will choose $\sim M$ in region II. Since $\sim M$ is the dominant strategy, players 1 and 2 need not consider the external cost parameters (δ^{oe} and δ^{be}). Given that the internal deltas are relatively low in relation to the advantage of defecting $[(\delta^{oi} + \delta^{bi}) < (1 - c)]$, D is the dominant strategy for players 1 and 2. Region I, on the other hand, represents a socially perverse outcome whereby players 1 and 2 always defect because the advantage of defecting $(1 - c)$ is greater than the sum of all delta parameters. At the same time, the monitor has a dominant strategy of M because the rewards received from detecting defection exceed the monitoring costs and are guaranteed to occur (assuming perfect detection), because players 1 and 2 face the dominant strategy of D.

This analysis demonstrates that (1) introducing norms and monitoring is not sufficient to change predicted results in a Prisoner's Dilemma base game and (2) a change in predicted results may not be socially beneficial. No set of equilibria exists where the actions of the monitor totally prevent defection. The only equilibrium region in which players 1 and 2 select a pure strategy of cooperating occurs where the value of the internal normative parameters exceeds the advantage of defecting (IV). The presence of a monitor who is motivated to select a mixed strategy boosts the level of cooperation in one region (III) but does not ensure cooperation. Moreover, the lower the probability of defection, the higher the monitoring rewards (R) need to be to offset the reduced probability of receiving the reward.

The results in Figure 1 hold because the reward to the monitor comes only if there is defection to be reported. If the monitor is rewarded specifically for monitoring, regardless of whether defection is discovered, there is an additional equilibrium region in which the monitor ensures cooperation. In this region, the reward for monitoring exceeds the cost of monitoring regardless of whether defection occurs. Thus the monitor chooses a pure strategy of M, which makes C a dominant strategy for players 1 and 2 whenever the sum of their internal and external delta parameters exceeds the advantage of D.[22]

Empirical studies and formal models suggest several other motivational schemes for monitors. Some motivate monitors by embedding them in a series of nested institutions that reward monitors who actively and reliably monitor with positive returns from the increased productivity that the rules generate (see Greif, Milgrom, and Weingast 1990; Milgrom, North, and Weingast 1990). Monitors may also be direct participants in ongoing relationships where efforts are made to reward one another for monitoring and to ensure that monitors participate in the greater returns that all achieve when temptations to defect are reduced. In such situations, monitors may achieve sufficient benefits from monitoring to induce a high level of conformance (but never 100%) in an isolated system without recourse to central authorities (Weissing and Ostrom 1991a, 1993). If one wanted to analyze the incentive structure found in many field settings where monitors are hired as external, disinterested guards, one could change the game so that the monitor receives a salary regardless of whether he or she catches a defection or shirks. In such a setting, the monitor has little incentive to monitor, so that the rate of cooperation depends heavily on the size of the internal delta parameters for players 1 and 2.

The Rules Game

Predictions that individuals will select C rather than D in the rules game rely upon (1) the base game payoffs, (2) changes in players' payoffs because of the addition of at least one delta parameter, (3) changes in the players' payoffs due to the addition of an objective and institutionally created consequence for breaking a rule, (4) the probability of detection, and (5) the probability of sanctioning.

The rules game shifts the norms game to a game with a rule backed by two norms. The rule that structures this game is, [Players 1 and 2] [must] [C] [always] [f]. The rule adds a fine (f) to the payoffs for players 1 and 2 for D if their defection is monitored and sanctioned. A sanctioning norm creates this fine and assigns the responsibility of imposing it on player 4: [Player 4] [must] [impose f on a defector] [when player 3 reports a D] [ ]. The rule is also backed by a monitoring norm: [Player 3] [must] [monitor] [always] [ ].

The monitoring norm adds delta parameters to player 3's payoffs. Whereas the monitor was rewarded only by prudential rewards in the norms game, now the monitor considers both prudential rewards and normative rewards as well as the costs of monitoring. The sanctioning norm adds another player, player 4 (the *sanctioner*), whose payoffs include delta parameters reflecting the sanctioning norm DEONTIC.[23] The cost of sanctioning is the only other parameter in player 4's payoffs. In other words, player 4 is a volunteer sanctioner "rewarded" solely by normative interests.

A wide variety of mixed strategy equilibria are possible that depend on (1) the relative expected value of the fine and the relative size of the external delta parameters for players 1 and 2, (2) the size of the reward and deltas associated with monitoring as compared to the costs of monitoring, and (3) the sign and size of the deltas associated with sanctioning minus the costs of sanctioning as compared to the value of the delta parameters for not sanctioning.[24]

We set aside the tasks of analyzing the many possible equilibrium regions of the rules game and focus here on the simpler task of establishing conditions for equilibria in which players 1 and 2 always cooperate. The rule adds a fine and a new player, yet the monitor still plays a crucial role. The parts of the game that come from the OR ELSE (the fine and the sanctioning norm) do not even enter the payoffs for players 1 and 2 unless the monitor chooses M.[25] As in the norms game, we assume that player 1 and player 2 do not see each other's choices and that the external delta components occur only when player 3 monitors.

Cooperation is a pure strategy for players 1 and 2 if and only if either of the following conditions is met:

$$\delta^{oi} + \delta^{bi} > 1 - c \quad \text{or}$$

$$[(\delta^{oi} + \delta^{bi}) + (\mathrm{p}(M)^{*}(\delta^{oe} + \delta^{be}) + (\mathrm{p}(S)^{*}f)) > 1 - c], \quad \text{where}$$

f = fine for breaking the rule

$\mathrm{p}(M)$ = probability of being monitored

$\mathrm{p}(S)$ = probability of being sanctioned.

The first condition is the same as in the norms game. The second condition figures in the effect of the monitor and sanctioner. The probability of monitoring [$\mathrm{p}(M)$] and sanctioning [$\mathrm{p}(S)$] depends upon the payoffs to player 3 (the monitor) and player 4 (the sanctioner). In order for the monitor and the sanctioner to be motivated, the values of following the monitoring and sanctioning norms have to be greater than the relative cost of doing their jobs. In the case of the sanctioner, the value of the delta parameters needs to be greater than the expense for imposing the sanction (E_s): $(\delta_s^o + \delta_s^b) > E_s$. The monitor balances both delta parameters and material rewards against the expense of observing players 1 and 2 (E_m). Player 3 monitors when $\mathrm{p}(D)R_m + \delta_m^o + \delta_m^b > E_m$, where $\mathrm{p}(D)$ is the probability that either player 1 or player 2 will choose D.

In settings where players develop a strong internalization of norms (high internal delta parameters), the presence of even a low-to-moderate sanction (f) may be sufficient when combined with reliable monitors and sanctioners [high $\mathrm{p}(M)$ and $\mathrm{p}(S)$], to encourage a high rate of cooperation. If players 1 and 2 expect either the monitor or the sanctioner to break their respective norms, then the expected probability of the sanctioner choosing to sanction [$\mathrm{p}(S)$] tends toward zero, and f drops out of the decision calculus for players 1 and 2. Even in a rule-governed game, if monitors are not motivated to monitor and sanctioners are not motivated to sanction, cooperation rests substantially on internalized norms of the players. Clearly, recognizing rules in formal analysis of dilemma situations does not automatically "solve" the dilemma and end analysis. Instead, adding rules suggests a whole new set of research questions. For example, How do changes in the level of internalization of rules (δ^{bi} and δ^{oi}) affect the levels of monitoring and sanctioning required to bolster cooperation at given levels of social pressure (δ^{oe} and δ^{be})? What size do external delta parameters need to be in order to ensure cooperation at various rates of monitoring and sanctioning with a given value of f? How do the incentives to monitor and sanction differ if we assume that players 3 and 4 are the same person? What are the empirical equivalents of delta parameters and OR ELSES in field situations?

MOVING BEYOND SYNTAX

Although our focus is mainly on the syntax of a grammar of institutions, the grammar of institutions, as is the case with other grammars, also includes semantics (i.e., the meaning of statements) and pragmatics (i.e., how to apply the syntax and semantics in practice). The delta parameters provide one example of the semantics of the grammar. Discussing how to operationalize the syntax components begins to address the pragmatics of the grammar. Analyses of many issues of interest to scholars of institutions will delve more deeply into pragmatic issues such as (1) the effects of configurations of institutional statements, (2) the consistency and completeness of the institutional statements of an institution, and (3) legitimacy and compliance.

Institutional Configurations

The descriptions of the components of rule and norm statements (except the OR ELSE) have focused on single statements as if the contents of the institutional statements were independent. The focus on single statements is for expository reasons only. When we examine the interactions of individuals in an empirical situation, we often find that a configuration of rules, norms, and shared strategies influences the choices of individuals at any one point of time. In fact, we often find institutional configurations, such as shared strategies and professional norms, nested within enforced government regulations.[26]

In some cases, the CONDITIONS component of an institutional statement explicitly states the linkages between statements in a configuration. For example, a rule permitting some action may state, as CONDITIONS of the rule, that the individual must follow a procedure outlined in another rule. In other cases, the linkage between statements is implicit. For example, the CONDITIONS component of a voting rule for legislation may not overtly make reference to the quorum rule, but the specific quorum rule in place strongly influences the effects of the voting rule. A rule stating that a majority must approve before a bill becomes a law differentially affects behavior depending upon (1) the quorum rule that states how many members must be present and voting for a vote to be legal and (2) the rule that states what happens if no positive action is taken (e.g., the OR ELSE rule that states whether the outcome is a return to the status quo or some other alternative).

Analyzing complex institutional configurations will require new capabilities. We are currently in the process of developing a classification system that identifies types of rules or norms according to their function in a configuration. Our current efforts integrate the institutional grammar with extensive work that outlines levels of action and the elements of an action situation that have been elucidated elsewhere (Kiser and Ostrom 1982; E. Ostrom 1986; Ostrom, Gardner, and Walker 1994).[27] The classification system seeks to enhance efforts to place individual institutional statements in the context of the mix of institutional statements that structure a situation.

Institutional Consistency and Completeness

When an institutional statement assigns a DEONTIC operator to an action or outcome, then that action or outcome becomes obliged, permitted, or forbidden for those with the listed ATTRIBUTES under the specified CONDITIONS. Thus, institutional statements with DEONTICS can be thought of as transformations that partition sets of possible actions or outcomes into subsets of obligatory, permitted, and forbidden actions and outcomes. Recognizing this set logic based on DEONTICS provides a tie to the works in philosophy of law and logic (Braybrooke 1994; Ellickson 1991; Gibbard 1990; V. Ostrom 1995; Tyler 1990; von Wright 1951, 1963). Moreover, the rigor of the logic-based system disciplines discourse by making inconsistencies more apparent.

Set logic, combined with the organizational capacity of the syntax, identifies inconsistencies and incompleteness in rule systems. We presume that most rule systems are incomplete. The existing rules do not cover all possible combinations of ATTRIBUTES, CONDITIONS, and AIM. The incompleteness of most existing rule systems becomes even more apparent when we recognize that the attributes and conditions of situations may be interpreted in different ways.[28] In complex situations, we presume that inconsistent rules develop; an action may be required under one rule and forbidden under another. Inconsistent systems may snare the unwary and trap others in Catch-22 situations. In political arenas, conflict plays an important role in generating information about inconsistencies. With effective conflict-resolution arenas, some inconsistencies can be tackled and resolved. Conflict also plays a role in addressing incompleteness. Much of political persuasion involves efforts by actors to convince others that new circumstances match the CONDITIONS of a rule that assigns a DEONTIC to the action that is in their best interest (McGinnis 1993).

Legitimacy and Compliance

If rulers enforce rules primarily by physical force and fiat, individuals subject to these rules are unlikely to develop internal deltas associated with breaking the rules; nor are external deltas likely to enhance the rate of rule conformance (Tyler 1990). If those who are supposed to follow a rule view it as illegitimate, they may even reward one another for actions that break the rules (a positive δ^{be}) instead of adopting the type of metanorm envisioned by Axelrod (1986) (a negative δ^{be}).

The complementarity of deltas and the OR ELSES emerges in the analysis of compliance. When delta parameters are close to zero, the costs of maintaining compliance through the OR ELSE drastically increase (Ayers and Braithwaite 1992; Levi 1988; Margolis 1991). Without a relatively high level of voluntary, contingent compliance to rules, Levi (1988) explains that rulers can rarely afford the continuing costs involved in hiring enough monitors and sanctioners, motivating them, and imposing sanctions in a manner sufficient to ensure that citizens conform to the rules rather than risk the chance of detection and punishment (see also Frey 1994). On the other hand, if violators can expect to reap the benefits of violating prescriptions without facing the probability of some established punishment (if there is no OR ELSE), then the experience of feeling like a "sucker" may erode the value of the delta parameters (Levi 1988, 1990; Mansbridge 1994; E. Ostrom 1990; Ostrom, Gardner, and Walker 1994).

The ADICO syntax illustrates the cumulative manner in which institutional statements affect individuals' expectations about the actions of others and the consequences of their own actions. We started this enterprise as an effort to define clearly the concept of rules and to create a grammar of rules.[29] We found that in order to do this we needed to clarify how rules were related to norms and shared strategies. Fitting this all into a syntax helped us to catch inconsistencies, which further tested and refined our understanding of each type of institutional statement and each component.

Delving deeper into each of the syntax components brought to light connections among these concepts that, at least for us, had not previously been linked. Further applications and developments of the grammar can strengthen our understanding of the link between institutions and theories of action. Attending to a grammar of institutions equips us to return to the core issues of institutions and political order with a respect for the complexities involved.

Notes

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1. Our concept of institutions parallels Gidden's (1979, 1984) concept of system. Another similarity to Gidden's work is our stress on the constitutive character of rules (see also V. Ostrom 1980). Our concept of situations draws on Popper 1967.

2. Calvert indicates: "There is, strictly speaking, no separate animal that we can identify as an institution. . . . 'Institution' is just a name we give to certain parts of certain kinds of equilibria" (1992, 18).

3. These are "forms of *talk about* experience" in the sense that Burke views the study of human relations in terms of linguistic instruments (1969, 317).

4. Chomsky defines the grammar of a language as the theory of that language (1957, 49). Burke explains his use of the term *grammar* as a theory to explain "what people are doing and why they are doing it" and uses a syntax composed of five terms that he posits to be the generating principles of his investigation (1969, xv).

5. This also applies to grammars in linguistic studies as Chomsky indicates (1957, 15; idem 1965, 11).

6. A formal syntax can be compared with that of other scholars. We have just learned about the Dalhousie logic of rules, which uses a syntax without an OR ELSE and requires that prescriptive statements be recast to use the forbidden DEONTIC operator (see Braybrooke 1994). Our concept of ATTRIBUTES is the same as the *wenn* component of the Dalhousie syntax.

7. The largest group to which a prescriptive statement could apply is the *folk* component in the Dalhousie system (Braybrooke 1994).

8. The logical relationships among the deontic operators include the following: (1) $D = P \cup O \cup F$; (2) $F \cap P = \varnothing$, $F \cap O = \varnothing$, and $O \cap P = O$; and (3) O implies P.

9. John R. Commons (1968) stresses the correlative nature of rights. To state that someone has a right, someone must have a duty to observe that right. The person with a right is permitted to do something, while those with a duty are forbidden or required to do something.

10. Tsebelis (1989, 1991) argues that in a game with only mixed strategy equilibria, increasing the size of the OR ELSE does not reduce the level of rule infraction but rather reduces the level of monitoring. Weissing and Ostrom (1991b) have shown that Tsebelis' results hold in many but not all cases.

11. The complexity of modern economies is so great that centralized law creation cannot effectively cope with the need to achieve normative regulation among communities of individuals who repeatedly face collective-action problems (Cooter 1993; Ellickson 1991).

12. Universal statements are relatively rare explanations for human behavior. The shared strategy that all rational players should adopt a dominant strategy or the norm that all actors are always obliged to tell the truth, exemplify unqualified institutional statements.

13. The logic of the delta parameter is similar to Etzioni's (1988) discussion of deontology.

14. Societies undergoing substantial "liberalization" could be thought of as developing shared understandings that individuals who had been forbidden to take certain actions in earlier times are now permitted to do so. When the new norm is shared, individuals who still attempt to obstruct the previously restricted actions now face a cost for breaking the new norm. Thus, changes in norms over time will be reflected both in the particular DEONTIC assigned to an action and in to whom the cost of breaking the norm is assigned.

15. Kreps and his colleagues do not assume that the basis for one actor playing tit-for-tat is necessarily the acceptance of a norm. They simply assume that some players only have available to them a tit-for-tat strategy or that there is some probability that one player's payoffs are such that tit-for-tat is strongly dominant (1982, 247). The latter condition would be the case if some players in a population have some combination of delta parameters associated with playing tit-for-tat whose values are high.

16. An appendix with the extensive form of each game is available from the Workshop in Political Theory and Policy Analysis at Indiana University.

17. The punishment in the grim trigger might cause one to wonder whether the retaliation is an OR ELSE. The shared strategy is not a rule, using the ADICO syntax, unless the prescription to punish someone who deviates is backed by another rule or norm. An institutional statement that could back a trigger rule might be a sanctioning rule such as, "All other players must defect for the rest of the game (or, in not-so-grim triggers, for *n* rounds) when one player defects in any of the rounds, OR ELSE the other players face the probability of a further sanction," and a monitoring norm that "all players must monitor all other players." Notice that the sanctioning rule changes the DEONTIC assigned to *C* from *permitted* or *obligatory* to *forbidden* in the CONDITION of a defection in the prior round. If the advice to cooperate to avoid a trigger response is not backed by monitoring and sanctioning institutions, the massive defection that is threatened by the trigger is either a prudent response to defection or a norm of retaliation.

18. It would, of course, be possible to include discount rates in the analysis, but we assume they are zero here to keep the focus on other questions and not those related to the size of the discount rate. For a discussion of the importance of discount rates in the analysis of cooperation, see Axelrod 1981, 1986.

19. It is possible to illustrate the addition of norms without a player who is assigned the specialized role of a monitor by simply assuming that players monitor each other (Weissing and Ostrom 1991a, 1993). To do this, however, one needs to model a sequential structure that introduces more complexity than we desire in this initial application.

20. Freelance reporters are an example of this type of monitor. They receive payment for detecting and reporting nonconformance with accepted norms. Rewards include fees for stories and increased probabilities of receiving prizes for good reporting.

21. A mixed strategy is a probability distribution over the pure strategies for a player. In a static game, one may view the mixed strategy as the probability of choosing one of the other pure strategies. One can also intepret mixed strategies as behavioral tendencies in a repeated context where the probability of choosing a pure strategy, say *C*, is viewed as a cooperation rate.

22. The extensive form of this game and the equilibrium diagrams are available from the Workshop in Political Theory and Policy Analysis at Indiana University.

23. The delta parameters for players 3 or 4 could be disaggregated into their internal and external components for an analysis that wished to focus on questions that distinguished between internal and external pressures on the monitor or sanctioner.

24. In this game, player 3 always correctly detects whether defection has occurred, and player 4 only has the option of sanctioning players who have defected. If players 1 and 2 cooperate, player 4 does not have a choice of whether or not to sanction. This eliminates issues of false detection and corrupt sanctioners from the current analysis. Future efforts will address questions related to monitoring and sanctioning errors.

25. One could argue that when prescriptions are rules, individuals will place higher values on the deltas than when

the prescriptions are norms (see Braybrooke 1987). If one assumes that the presence of rules influences the internal deltas, that δ^{oi} are higher in the rules game than in the norms game, then the rule would influence the structure of the game even when the monitor fails.

26. See E. Ostrom 1986 for a discussion of the configurational aspect of rules.

27. The element of the action situation that best fits the contents of the AIM of the prescription is the first part of the classification; the match between the elements and the CONDITIONS provides the second part of the classification. Further refinements in the classification focus either on the other components in the institutional statements or on creating subdivisions within the basic categories provided by the elements of an action situation.

28. For a philosophical discussion of uncertainty and incompleteness in the applications of rules, see Heritage 1984.

29. For earlier efforts to address the concept of rules, see Black 1962, Collett 1977, Ganz 1971, and Harré 1974.

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THE ADVANTAGE OF BEING MODERATELY COOPERATIVE

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We use computer simulation to identify a process by which cooperation evolves without iteration, and evolves better in large than in small societies. It is based on an empirically supported heuristic for deciding whether to enter noniterated prisoner's dilemma games, namely, Expect others to have the same dispositions as yourself. *Players are assigned a probability of cooperating that also defines their expectations about others' behavior and thus their willingness to play. The carrying capacity of the ecology is 10,000. Players multiply by 2 if their aggregate payoff in a given round (1) places them among the more successful 5,000 and (2) is more than zero. We find that the most adaptive disposition is toward the mean of the population. That is where individuals have the optimal mix of consummated plays with more cooperative players and unconsummated plays with less cooperative ones. When encounters occur by proximity, fortuitous clusters toward the cooperative tail will grow and dominate the society. Such clusters are more likely in large societies.*

How a person has behaved in the past can often predict how that person will behave in future. And if we do not have first-hand experience, then rumor, reputation and gossip—stories about other peoples' experience with particular people—can be useful substitutes. But what if we have to decide about strangers, people with whom we have no experience, first- *or* second-hand? One solution, of course, is to avoid them; the warning to avoid strangers is not only for children. When that is not possible, we can depend on stereotypes ("White males can't be trusted") or rules of thumb ("Never trust anyone over thirty"). If such heuristics have an element of truth to them, so much the better; but if they do not, then at least they provide a basis for action with respect to different kinds of people.

Orbell and Dawes (1991) have proposed a heuristic for deciding whether to interact with strangers that draws on the judge's own characteristics, not those of the target. The heuristic is, Expect potential partners to have the same behavioral intentions as yourself. If we have no direct information about a person, we can, in other words, treat our own intentions as diagnostic of what to expect from that person. If we intend good behavior, then we will anticipate good behavior from a potential partner. If we intend bad behavior, then we will anticipate bad behavior and refuse to have anything to do with that person. We use computer simulation to answer two questions: (1) Does making decisions about strangers by this heuristic benefit or hurt people who are disposed to cooperation, or does it make no difference? and (2) Does the manner in which encounters under this heuristic are structured affect the evolution of cooperativeness in the society? First, we find the heuristic provides a relative advantage to individuals who are moderately disposed to cooperation, not those toward either of the extremes. Second, we find if encounters happen by proximity—while selection happens on a societywide basis—descendants of those who were originally at the mean of the *most* cooperative neighborhood come to dominate the entire ecology.[1]

The findings complement the more standard ones about how cooperation can evolve through iterated play (e.g., Axelrod 1984; Kollock 1993; Lomborg n.d.; Trivers 1971; Vanberg and Congelton 1992). Iterated play is not necessary for cooperation to survive and prosper. And because the probability of there being *some* very cooperative clusters increases with the size of the population, the findings show how cooperation can do better in large than in small societies (see Buchanan 1965; Olson 1965).

Simulation is an appropriate tool for developing the systemic implications of behavioral assumptions, but the validity of a simulation's findings depends, of course, on the validity of the behavioral assumptions in question.

BEHAVIORAL ASSUMPTIONS

The folk adages that "you should put yourself in the other person's shoes" and that "you should ask yourself what you would do in the same situation" provide a priori ground for expecting people to adopt the heuristic in question. More systematically, as Orbell and Dawes (1991) point out, the heuristic is consistent with the extensive literature on false consensus that documents our propensity to expect others to be like ourselves (e.g., Marks and Miller 1987; Ross, Greene, and House 1977) and with the much-replicated finding from laboratory research that people expect others to choose between cooperation and defection in the way they, personally, choose.

Finally, Orbell and Dawes (1993) have themselves reported laboratory evidence supporting predictions from the heuristic. If people do base their expectations of others' behavior on their own intentions, then those who intend cooperation will be more optimistic about others' intentions and will enter prisoner's dilemma games more frequently than those who intend defection. The authors compare behavior in a condition where subjects must choose only between cooperation and defection with behavior in a condition where subjects have, in addition, the choice of not playing. By projecting from the former condition to the latter, they show that the incidence of opting out among "intending cooperators" is significantly smaller than among "intending defectors" consistently across five different decisions.[2] The data do not prove the existence of the heuristic; other processes might produce the same effect. But the authors reject the most plausible alternative explanation, namely, that subjects can recognize the intentions of potential partners and avoid intending defectors accordingly.

We have elsewhere tested the robustness of the heuristic against "perceptiveness," a capacity that (for a modest price) lets individuals recognize others' intentions, and it does very well indeed (Orbell, Runde, and Morikawa 1995). The heuristic is, after all, cognitively cheap, and as Orbell and Dawes (1993) point out, it will produce correct estimates for most people most of the time.[3] We speculate that this heuristic is the product of an evolutionary trade-off between the advantages that would accrue from a more reliable method of predicting others' behavior and the costs of producing such a mechanism. Projecting from one's own intentions certainly is simple. Perhaps the inevitable errors from doing so are not as damaging as the cost of developing a more accurate mechanism for recognizing others' intentions.[4]

STRUCTURE OF THE SIMULATION

While we are not attempting to simulate any particular society, we *are* attempting to capture a basic feature of all societies—that they comprise for their members a field or ecology of relationships with other members, relationships that are available but that may or may not be consummated. Such relationships may, of course, take the structure of any of the diverse games that have interested game theorists over the years, but we confine ourselves to the prisoner's dilemma because it captures two features of an extraordinarily wide range of human relationships—that they are potentially productive and that they involve the risk of exploitation.

Simulation makes it possible to isolate the effects of the heuristic that concerns us from the variety of other factors that might influence the evolution of cooperativeness. One of these is *experience*. Standard analyses allow for iterated relationships among players, a factor that allows one to respond to another's defection with retaliatory defection (Axelrod 1984) or with avoidance (Vanberg and Congleton (1992). Yet the heuristic in question does not depend on iteration and we accordingly provide our players with only one-shot relationships.

Another factor is *power*. Clearly, power imbalances (e.g., between genders, class, ethnic groups) do affect relationships in the natural world, perhaps placing some individuals disproportionately into cooperator roles while leaving others free to defect. As we shall show, our simulation does differentiate people in terms of their propensity to cooperate, and such differences might map onto natural world categories; there has been much speculation about gender in this respect, for example. Yet power imbalances per se do not bear on the working of the heuristic, and our simulation methodology lets us make all individuals equal except in their cooperative propensities.

The simulation involves a population of players, each of whom encounters other members of that population in circumstances allowing, if both agree, a prisoner's dilemma game between them. Each player is assigned a probability of cooperation should a relationship be consummated and, consistent with the heuristic in question, that probability also defines each player's expectations of cooperation from a potential partner. The payoff matrix for all games is

$$\begin{array}{cc} 1,\ 1 & -2,\ 2 \\ -2,\ 2 & -1,\ 1 \end{array}$$

with the reversion for an unconsummated play being zero. Notice that playing with a cooperator will always be profitable, while playing with a defector will always be costly—regardless what one's own choice between cooperation and defection might be.[5] Accordingly, the rationality of the play-versus-not-play choice depends critically on what one expects a potential partner to do.

Encounters continue throughout a series of rounds, with individuals' payoffs being summed and compared at the end of each round. Two selection rules decide which players will "die" and vanish from subsequent rounds and which will "multiply" and have their *offspring* populate those rounds. Our interest is in the changing complexion of the population throughout subsequent *generations*, in particular in the relative success of individuals with different "propensities to cooperate."

Cooperation is, of course, a dominated response in the prisoner's dilemma, meaning that *no* propensity to cooperate (at least, greater than zero) is rational. This does not represent a particular problem for our model, however. The extensive literature on the prisoner's dilemma is, in large part, about solutions to the problem, and an individual's propensity to cooperate may be thought of as showing the extent to which a given solution or set of solutions works to promote cooperation for that individual. Solutions will rarely be perfect. Campbell's (1975) discussion of normative systems, for example, proposes that behavior will characteristically fall between the extremes demanded by socially evolved rules and biologically evolved egoistic impulses (see also Ullmann-Margalit 1977).[6] Because of that imperfection, we should expect variation among individuals in their characteristic behaviors and also variation within individuals across their diverse encounters. Both types of variation are, of course, everyday realities.

In our simulation, the relevant environment for each individual is the set of other individuals in the population, and—because adaptiveness is defined relative to particular environments—we examine the adaptiveness of various cooperative dispositions across three different distributions of dispositions. We are thus able to explore whether the same dispositions are adaptive in all environments or whether some are adaptive only in some environments and some only in others.

Our simulation also lets us examine the welfare implications of the heuristic in question. By definition in the prisoner's dilemma, mutual cooperation produces more social wealth than any other outcome, meaning that, ceteris paribus, a society's wealth increases with the number of its members who have high dispositions to cooperate. If the heuristic supports generally cooperative dispositions, therefore, it will be socially productive. If it supports generally defecting dispositions, on the other hand, it will be socially damaging.

The Population

Our simulation starts with 10,000 individuals scattered across a 100-by-100 lattice of *niches* with, initially at least, one individual in each niche. The ecology of niches is structured like a round ball with all of the edges wrapped around and linked to the opposite edges. This structure means that wherever it is located, each niche has eight *neighboring* niches.

Each individual is defined in terms of a *base rate of cooperating* (BRC)—a probability that can be understood, as we have suggested, as that individual's response to the variety of other mechanisms that support cooperation in that particular society. We investigate three distributions of such cooperative dispositions: a *nasty* society with 1,600 individuals in each cooperativeness distribution decile from .1 to .5 and 400 in each decile from .6 to 1.0;[7] a *flat* society with 1,000 individuals in each decile from .1 to 1.0; and a *nice* society, with 400 in each decile from .1 to .5 and 1,600 in each decile from .6 to 1.0. (The mean "dispositions to cooperate" are .4, .55, and .7, respectively.)

Encounters

An encounter provides two individuals the opportunity of playing a prisoner's dilemma game with each other. Consistent with the heuristic in question, each individual in an encountering pair decides whether to play as a function of its own BRC. The computer sets the odds of the individual's choosing *play* by the specified odds of the individual's choosing *cooperate* should the game be consummated (i.e., should both individuals choose to play).

For example, an individual with the cooperative disposition of .70 assigns that same high probability to an encountered individual's also cooperating.[8] Despite its nice disposition, however, this same individual might choose not to cooperate should this particular game be consummated; that choice would be made with a .3 probability. Alternatively, an individual with the uncooperative disposition of .2 might, despite relative pessimism about others in general, anticipate cooperation from this particular individual and choose to play.

If the encounter results in a consummated play, each individual's payoffs are specified in the usual way from the matrix. If either individual refuses play and the encounter is not consummated, each individual makes nothing. In unconsummated relationships nothing is gained, but neither is anything lost.

Encounters happen randomly, but the population from which potential partners are drawn differs between two versions of the simulation. In one version, an encountered individual is drawn from the entire (starting) population of 10,000: anyone in this ecology can encounter anyone else. In another version, an encountered individual is drawn only from the eight niches that border on the individual's own—that individual's neighborhood.

Evolution

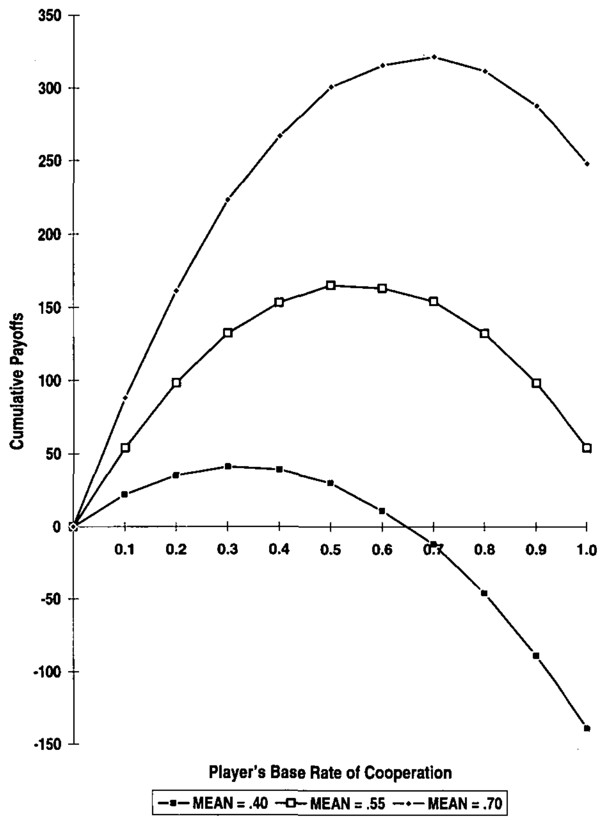
A generation consists of 100 million encounters among the 10,000 players. This means that each individual has, on average, 1,000 encounters in a given generation and that each individual can expect, during such a generation, to encounter about one-tenth of the players in the population. At the end of each generation, an individual's wealth (the sum of its payoffs across all encounters in that generation) determines its fitness. The individual may suffer genotypic death and consequently be unrepresented in the subsequent generation or may multiply, having two offspring with the same particular propensity to cooperate in the same niche in the next generation.

Individuals must meet two criteria if they are to multiply. The first is that their aggregated wealth must be greater than zero. This is to reflect the fact that *some* level of success is necessary for simple survival, and zero is a convenient criterion, consistent with the intuition that absent consummated play, the status quo should prevail (for discussion of relevant alternatives, see Orbell and Dawes 1993; Orbell, Schwartz-Shea, and Simmons 1984). Notice that with the zero payoff for unconsummated play in the payoff matrix, this criterion implies that an individual must have at least *some* consummated plays with cooperators if it is to survive and multiply.

The second criterion is that an individual who succeeds must, given the ecology's carrying capacity of 10,000, be among the wealthiest 5,000 in the population. If the above-zero payoff were the only criterion, members of a very successful population might *all* multiply with consequent pressure on available resources. As Malthus and subsequently Darwin recognized, animals' capacity to reproduce themselves is so great that competition for available resources is sooner or later inevitable. This second criterion is included to reflect the inevitably of that competition.

FIGURE 1

Mean Cumulative Payoffs in Three Societies (No Evolution, No Neighborhoods)



Notice that if more than 5,000 fail the above-zero criterion in a given generation, the population will fall below 10,000 in the next. Potentially, a society whose members are chronically unsuccessful can die out altogether.

FINDINGS

Simulation 1: No Evolution and No Neighborhoods

Figure 1 reports the mean cumulative payoffs at the end of a single round for players in each of the 10 BRC categories in each of the three societies. It is apparent, first, that cumulative payoffs are consistently higher in the more cooperative society. This greater wealth is to be expected because more plays will be consummated there than in the other two, and more of those consummated plays will have wealth-producing cooperate-cooperate outcomes.

More interesting, for each of the three societies, the most successful individuals are those whose BRC is about average for their society. They are more successful than the extreme defectors who would have won without the option of not playing. They are also more successful than the extreme cooperators who would have won if (granted the option of not playing) players were able to recognize others' intentions.

Why should this be the case? Because individuals at the mean are optimally situated to benefit from encounters with relatively cooperation-prone individuals while simultaneously being protected from relatively defection-prone individuals. The pattern is the same for all three societies, and for brevity we report data only for the flat society (with a mean BRC of .55). Figure 2 shows how well members of each BRC category in that society did in their encounters with members of each other BRC category. The top of the column to the extreme right, for example, shows that the 1,000 individuals with a 1.0 probability of cooperating in this society received, on average, a comfortable net gain from their encounters with other cooperative individuals like themselves. They also received a net gain from encounters with those in the .9, .8, and .7 categories. These gains, nevertheless, get progressively smaller because individuals in these categories are progressively less likely to play (thus returning their 1.0 cooperative neighbors only zero) and less likely to cooperate if they do play (thus returning the 1.0 cooperators at least occasional sucker's losses).

On the other hand, these 1.0 cooperative people take a net loss from their encounters with people whose BRC values are .6 BRC and below—a loss that is, in aggregate, greater than their net gain from the more cooperative elements in the society. There are, presumably, *some* cooperate-cooperate relationships even with the .1 BRC folk, but these are too few to make up for the sucker's payoffs they receive from most consummated relationships with such people.

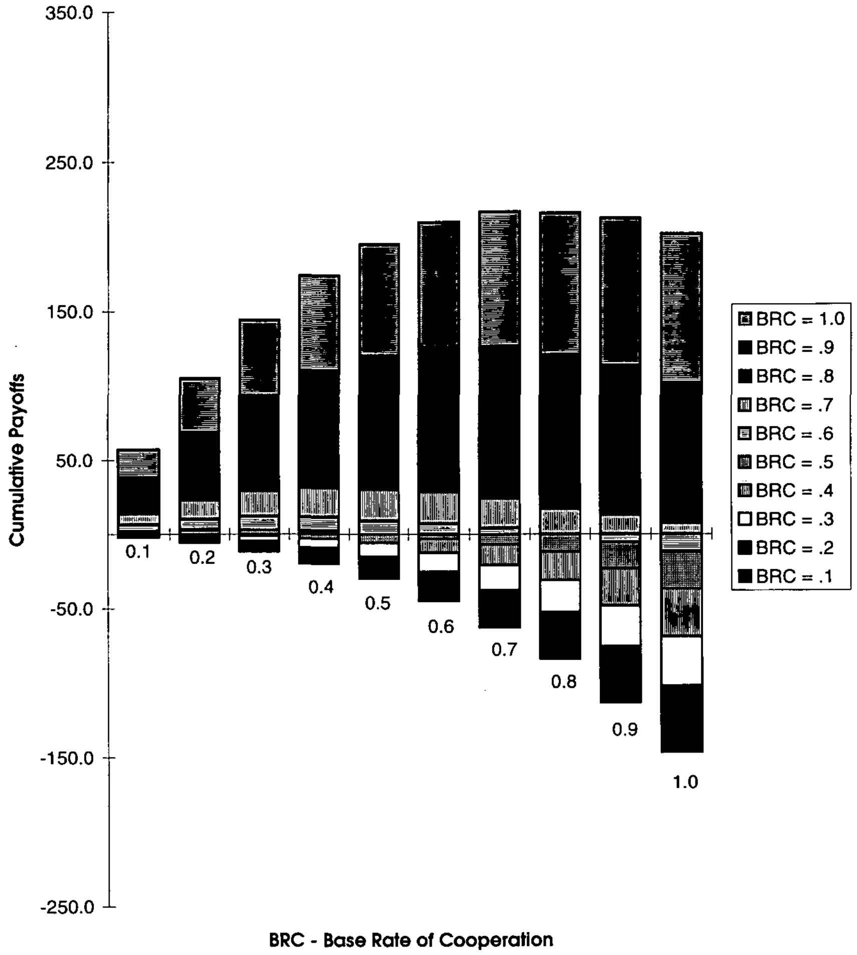
Notice that the losses these 1.0 cooperators take from such unpleasant relationships are, nevertheless, kept lower than they might be by the reluctance of the less cooperative people to play at all. In general, many encounters with players less cooperative than oneself will result in a zero payoff because of this reluctance, not in the sucker's loss they would endure had they been obliged to play. That is not as good as it might be if these people were more cooperative but not as bad as it would be if they were more willing to play. By comparison, those in the lowest BRC category (.1) have a much more favorable net of gains over losses—sufficiently favorable to give them, in net, a slim margin of profit.

This net of gains over losses is most favorable for individuals in the .5 BRC category close to the mean. This is because (1) to the right of the mean, net losses increase exponentially through progressively greater exposure to the sucker's payoff (by progressively greater willingness to play) and (2) to the left of the mean, net gains decrease exponentially as a consequence of progressively less exposure to the benefits of free-riding on cooperative plays (by progressively less willingness to play). Individuals around the mean do not do as well as they would if all plays were consummated, and if all those plays were with cooperators. But they do better, in net, than anyone else.

The same general considerations make the mean

FIGURE 2

Cumulative Payoffs for Players with Different BRC Values, by the Origin of Those Payoffs Population Mean = .55



Note: Population mean = .55.

dispositions in the other two distributions also the most profitable ones. Notice that this finding is dependent on the symmetry of the prisoner's dilemma matrix we use in these simulations. If, for example, the sucker's loss were worse than it now is (and other values were constant), then unsuspicious cooperatively inclined people toward the right of the distribution would lose more than they now do from each encounter with a defector, and the highest net payoff would move to the left of the mean. It would also move to the left of the mean if the free-rider's payoff were better than it now is. Conversely, the highest net payoff would move to the right of the mean if the payoff to mutual cooperation were raised or if the sucker's payoff or the free-rider's payoff were lowered. There are definitional limits to our capacity to shift around prisoner's dilemma payoffs in this manner, however, and matrix considerations do not change the more general finding that being *moderately cooperative*—relative to the rest of one's society—is the most profitable disposition in these simulations.

Simulation 2: Evolution without Neighborhoods

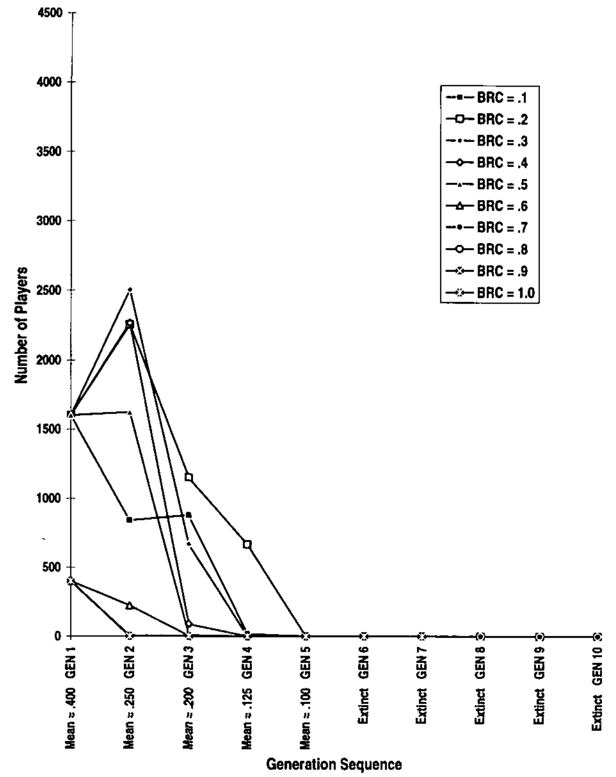
Figure 3 shows what happens when there is evolutionary selection through a succession of generations in the nasty society: everyone dies by the fifth generation. Individuals at and around the initial mean enjoy a peak in their fortunes after a few generations, but it is a peak that cannot be sustained. The earlier death of more cooperative elements in the society removes a source of profit for such initially successful individuals, and they, too, finally disappear. Essentially the same process happens in the flat society; the only difference is that it takes a little longer (until the eighth generation) for that society to die.

Notice that as various groups disappear, the society's mean BRC shifts, so that the most adaptive BRC is not constant through the succession of generations. Dispositions that work best at one stage of a society's history do not work best at a different stage.

As shown in Figure 4, however, the nice society avoids the self-reinforcing decline that happens in the

FIGURE 3

Evolutionary Model with No Neighborhood Effect I



Note: Starting population mean = .40.

other two. As in the other two, individuals at and around the initial mean do quite well at first, but here they manage to sustain that initial success—and, in doing so, they provide enough cooperative relationships to keep alive at least some members of several other BRC categories. It turns out that there is a threshold involved here. A series of simulations showed that given the other parameters we have adopted, the mean BRC of a society must be between .638 and .640 in order for the success of the mean to ensure the continued existence of the society. Below that threshold there are just not enough wealth-producing cooperators around.

Simulation 3: Evolution with Neighborhoods

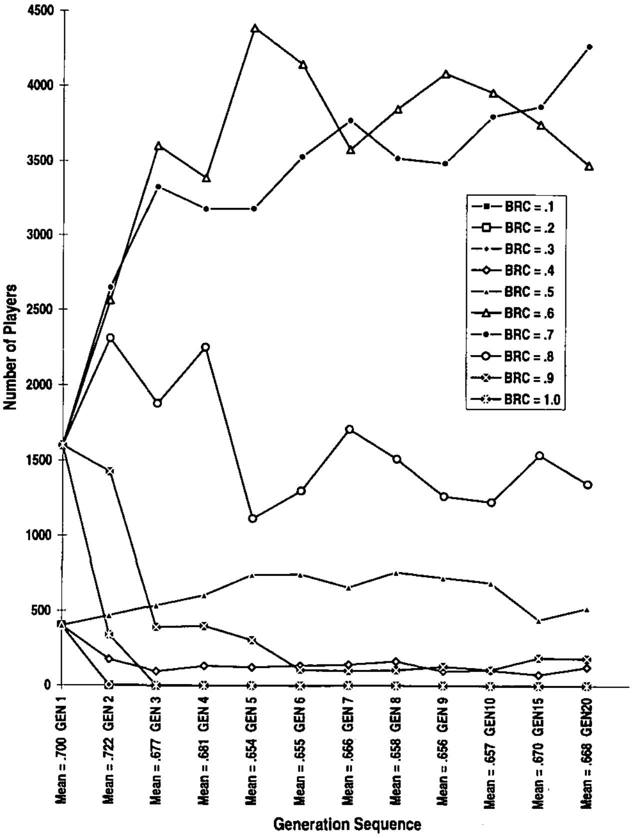
With due allowance for small-number instability, our finding about the relative success of individuals around the mean BRC in a society should operate within neighborhoods as well as the wider society. In other words, after any given generation the most successful individuals within a particular neighborhood should be those at or around the mean of that neighborhood.

The data support this prediction in all three cases, and we illustrate with the nasty society (with a mean BRC of .40) which, absent neighborhoods, died out completely by the fifth round. Table 1 reports, for that society, mean payoffs for a starting round as a function of individuals' own BRC and the mean BRC of their particular neighborhood. From the top row, for example, we can see that most successful individuals in the nastiest neighborhoods (with a mean BRC between .1 and .2) were the five whose BRC (.1) most closely matched the mean BRC in the neighborhood. Their mean payoff is less than zero (−8.8), meaning that (by the first selection criterion) they would not survive into the second generation. Yet their payoff is still better than any of the other 16 individuals in such neighborhoods—most obviously, better than the two perfectly cooperative individuals (BRC = 1), who lost on average 204.

In the nasty society, of course, we should not expect to find many neighborhoods whose mean BRC values are toward the most cooperative tail. The chance of any individual finding him- or herself in such a neighborhood declines with distance from the societywide mean; in general, the largest *numbers* will be located in the neighborhoods whose mean BRC is at or around that societywide mean. Similarly, we cannot expect individuals at the mean of their particular neighborhood always to be the most successful

FIGURE 4

Evolutionary Model with No Neighborhood Effect II



Note: Starting population mean = .70.

TABLE 1

Mean Payoffs

	PLAYER'S BASE RATE OF COOPERATION									
AVERAGE BASE RATE OF COOPERATION IN NEIGHBORHOOD	.1	.2	.3	.4	.5	.6	.7	.8	.9	1.0
≥0.1 <0.2	**−8.8** (5)	−23.0 (5)	−44.0 (3)	−85.0 (1)	−81.0 (4)	0.0 (0)	0.0 (0)	−126.0 (1)	0.0 (0)	−204.0 (2)
≥0.2 <0.3	**−1.5** (135)	−8.4 (167)	−19.2 (150)	−33.1 (205)	−52.9 (191)	−84.9 (38)	−110.0 (41)	−148.6 (42)	−200.4 (45)	−234.8 (40)
≥0.3 <0.4	11.2 (619)	**16.3** (653)	14.0 (596)	6.3 (618)	−11.4 (606)	−29.6 (146)	−67.0 (162)	−100.2 (149)	−153.2 (168)	−198.9 (162)
≥0.4 <0.5	28.2 (604)	46.6 (571)	59.2 (618)	**64.9** (580)	53.0 (582)	35.5 (162)	14.1 (143)	−15.8 (154)	−58.6 (150)	−121.2 (151)
≥0.5 <0.6	48.0 (212)	86.7 (169)	117.3 (204)	132.1 (174)	**136.4** (190)	127.9 (49)	119.7 (50)	104.2 (44)	63.7 (31)	1.2 (39)
≥0.6 <0.7	73.7 (23)	122.3 (35)	174.4 (28)	227.6 (19)	225.7 (26)	249.8 (5)	**299.3** (4)	231.3 (10)	185.3 (6)	80.0 (6)
≥0.7 <0.8	101.5 (2)	.0 (0)	207.0 (1)	**316.3** (3)	237.0 (1)	.0 (0)	.0 (0)	.0 (0)	.0 (0)	.0 (0)
≥0.8 <0.9	.0 (0)	.0 (0)	.0 (0)	.0 (0)	.0 (0)	.0 (0)	.0 (0)	.0 (0)	.0 (0)	.0 (0)
≥0.9 <1.0	.0 (0)	.0 (0)	.0 (0)	.0 (0)	.0 (0)	.0 (0)	.0 (0)	.0 (0)	.0 (0)	.0 (0)
Total	1,600	1,600	1,600	1,600	1,600	400	400	400	400	400

Note: One generation; population mean = 4. The top figure in each cell is the average payoff and the bottom one is the number of cases. Numbers in bold are the highest payoffs in particular neighborhood types.

in those areas; we should expect random fluctuation from that prediction—all the more so because of the relatively small numbers involved in each neighborhood.

Nevertheless, the pattern is the same in all types of neighborhoods in this society, and it persists in the flat and nice societies as well.[9] The question is therefore, How does this success of individuals at the mean of their particular neighborhoods work itself out through generations in an evolutionary sequence?

The answer is, It works to promote the more cooperative elements and thus to the substantial benefit of the society as a whole. We use the nasty society for illustrative purposes, once again. Figure 5 shows mean payoffs to individuals with different BRC values throughout an evolutionary sequence in which encounters are structured by neighborhoods. Most dramatically, far from dying out, this originally nasty society becomes progressively nicer until, by the end of the twentieth generation, more than 8,000 of its 10,000 members are from the .9 BRC category, and substantially more than 1,000 of the others are from the perfectly cooperative 1.0 category.

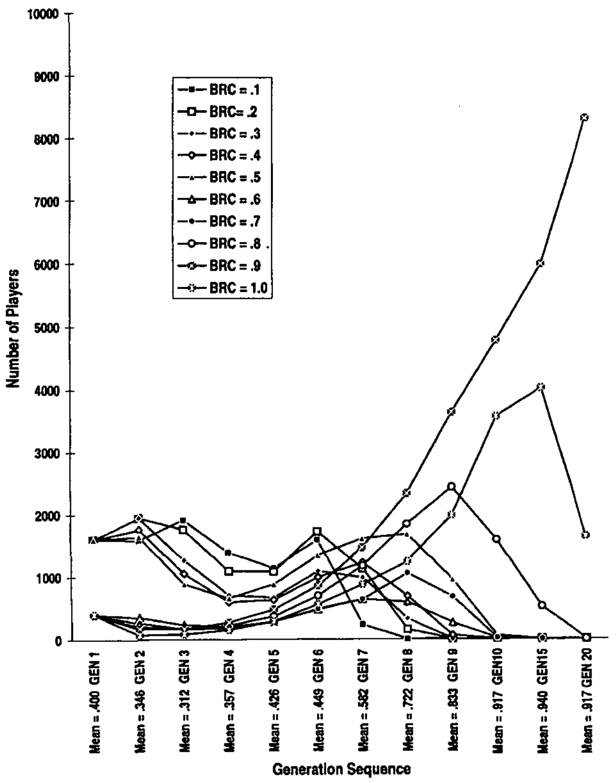
This progressive movement from nastiness toward almost total niceness is reversed only once, and then only momentarily. Between the second and the third generations, numbers of the more cooperative elements who find themselves distant from the mean of their particular neighborhoods do not survive, and their death lowers the social mean. Subsequently, however, the relatively few more cooperative individuals at the mean of more cooperative neighborhoods begin outcompeting less cooperative individuals elsewhere.

What is happening can be seen by returning to Table 1. There are, for example, 618 individuals with a .4 BRC whose neighbors average between .3 and .4, numbers that are not surprising, given the nasty complexion of this society. Consistent with our general finding, these individuals are far from the least successful (the least successful are the 162 with a BRC of 1.0 who lose, on average, 198.9). The average payoff for the 618 is, nevertheless, only 6.3—sufficient to let them survive the above-zero criterion, but only just.

On the other hand, four individuals with a .7 BRC find themselves—very unusually for this society—surrounded by neighbors whose mean BRC is between .6 and .7. These fortunate cooperation-disposed individuals do very well indeed, making on average 299.4. This is not only well above the zero cutoff for reproduction, but it is also a higher score than all but *three* of the 8,254 individuals in this society who are less prone than they are to cooperative behavior. Had we allowed reproduction to depend on relative success in a given generation, de-

FIGURE 5

Evolutionary Model with Neighborhood Effect



Note: Starting population with mean = .40.

scendants of these four would have comprised a large proportion of the next generation. As it is, they will have eight descendants in the next generation, and those eight will be well situated to do even better than their four parents in the evolutionary sweepstakes next time around.

Thus the process is ratcheted. As individuals at the mean of the relatively more cooperative neighborhoods outcompete less cooperative individuals elsewhere, the ecologywide incidence of the former goes up, and so does the absolute payoff required for being among the most successful 5,000 who will reproduce into the next generation.

An upper limit to this ratcheting process is set, of course, by the initial distribution in the society, and some societies will never get as far as others. In our nasty society, for example, there are *no* individuals located in neighborhoods with a mean BRC above .8, and only seven in the neighborhoods with a mean BRC between .7 and .8. By comparison, the nice society (mean BRC = .7) has 61 individuals in neighborhoods with a mean BRC between .9 and 1.0, of whom six have personal BRC values of .9 and make the largest score of any across the entire ecology (719.3). Nevertheless, this ratcheting process means that with neighborhoods and with the evolutionary process we have specified, societies will eventually be populated exclusively by individuals who are toward the most cooperative tail of the initial BRC distribution.

DISCUSSION

At the conclusion of such a sequence, of course, surviving members of our societies will not be distributed (as the starting population was) with one individual per niche. A three-dimensional topographic map of the ecology would show many individuals concentrated in relatively few niches. Surviving individuals will all be descendants of a very small number of ancestors—perhaps only one—and those descendants will all be located in the few niches that were originally occupied by those ancestors.

We do not pretend that this is how things would work out in any particular natural situation. In most natural situations, migration would be possible, meaning that—perhaps like successive waves of our remote *Homo erectus* or *Homo sapiens* ancestors—populations would overflow from the niches that provided for their initial success to populate ever more remote niches, competing with, and perhaps eliminating, the occupants of those niches.

While modeling migration would, no doubt, be challenging and interesting, letting successful individuals migrate would not change our essential findings. We might model it as a subset of a successful population leaving its home niche and moving into surrounding areas, in which case the competitive position of that population would not differ from what it is in the present model, absent migration. Alternatively, we might model it as *individuals* leaving their home niche, in which case migration would expose individuals to encounters from a wider and wider population, meaning that the mean BRC of the population from which its encounters came would approach more and more closely the mean BRC of the wider society. At the extreme, when any individual could migrate anywhere, the society's mean would define the effective environment for every individual, and the situation would be, for practical purposes, the same one that we modeled in the first of our simulations. In either case, adding migration would simply add complexity without improving our understanding of the basic processes involved.

Further, in most natural situations, niches have finite carrying capacities as much as do whole ecologies, meaning that for greater realism, we would have to establish an upper limit on the number of individuals who could occupy a given niche at a given time. Individuals certainly do compete for control of resources in particular niches, and such resources are inevitably limited. Limiting the carrying capacity of niches, however, would mean that when the carrying capacity of their niche was reached, successful individuals would be in competition with others who shared their own BRC. In our simulation, all occupants of a niche are cloned descendants of some single ancestor, meaning that the BRC complexion of the niche could not change through an evolutionary sequence nor, therefore, could the complexion of the wider society. Our concern is with the relative success of individuals with different BRC values, and studying that requires diversity of such values, at least initially. Our simulation provides diversity at the societywide level, and incorporates niches for the sole purpose of studying the effects of structuring encounters by proximity. Making competition happen at the level of those niches would add realism (of a kind) at the price of eliminating the diversity that is critical for any evolutionary process to proceed.[10]

Our modeling, therefore, has involved a compromise between capturing the complexity and interest of natural processes and providing sufficient conditions for identifying the evolutionary consequences of the heuristic in question, ceteris paribus. We have chosen to have individuals encounter each other via neighborhoods because that lets us take account of the fact that, even in relatively nasty societies, there will be *some* fortuitous clusters of relatively nice individuals, and because—as has been known at least since Darwin—isolation matters for the development of low probability variants. We did not make that choice from any attempt to capture the full complexity of how the heuristic might work in natural circumstances.

CONCLUSIONS

We have not tried to explain the heuristic of projecting one's own intentions onto prospective partners—although, as we have speculated, it might be part of our cognitive arsenal because of its fitness implications. The question we have asked, instead, is whether the heuristic has implications for the evolution of cooperative behavior. Perhaps a rule that provides a selective advantage to individuals who adopt it also provides a selective disadvantage to individuals who cooperate.

Our simulations suggest that it supports cooperation. The first shows that those who are modestly cooperative relative to others in their society do best. In one-shot games with an obligation to play, those inclined to defect would do much better than the former. But when people do not *have* to play these games and when they make the choice between playing and not playing by the heuristic we have been discussing, all others—even the most consistent defectors—are outcompeted by those in the middle range of cooperativeness. This is not an overwhelming triumph for cooperation; the most cooperative individuals are among the first to go. But it is a substantial advance.

A still greater advance comes when there is an evolutionary sequence with encounters structured by proximity. Whatever the incidence of cooperativeness in the wider society, there will normally be at least some fortuitous clusters of individuals who are very cooperative relative to that wider society. Structuring encounters by proximity means that members of such relatively cooperative clusters who are themselves moderately cooperative *relative to the other members of the cluster* will do best in those places. And because payoffs to such individuals will be high (they will have lots of plays with others who are cooperatively inclined), it also means that they will compete successfully in the wider society. The process will continue, moving the society in a more cooperative direction, until the population's complexion is defined exclusively by the descendants of such initially well placed cooperators.

The process we have identified is unusual in the literature insofar as it does not depend on iterated relationships to support cooperation. Sustained relationships among individuals are most explicitly required by tit-for-tat (Axelrod 1984) and various elaborations on that strategy (e.g., Kollock 1993; Lomborg n.d.; Vanberg and Congleton 1992), but they are also required by reciprocity (Alexander 1979; Trivers 1972) and, implicitly at least, by most solutions that rely on coercion and the anticipation of coercion. In our simulations, however, individuals meet each other once only—or, more critically, they have no memory of how things worked out in the past if a given individual *is* encountered twice.

We do not challenge the importance of iteration in many natural situations; obviously, experience with other people is very important in human affairs. But it is not the only basis from which we can make judgments about each other, and our study has identified a process by which cooperation does well in societies of strangers, where people have no knowledge about what to expect from potential partners other than the "knowledge" they bring about their own behavioral intentions (for a rare argument that does not require iteration, see Nowak and May 1993).

It is also unusual insofar as it works to support cooperation more effectively in large than in small societies. Even if its population is initially quite uncooperative, a large society will almost certainly have at least a few clusters toward the cooperative tail of the distribution, and individuals at the mean of those clusters will, sooner or later, outcompete everyone else. On the other hand, a very small society is less likely to have many clusters that deviate far from the mean (Dahl and Tufte 1973), with the result that its population will not evolve much more cooperativeness than it had at the start. Many arguments propose that cooperation can do quite well in small societies but that it becomes disadvantaged as the society's size increases (e.g., Buchanan 1965; Olson 1965). The process we have identified depends on encounters happening within small neighborhoods, but when competition happens societywide among members of such clusters, it produces the opposite conclusion.

While isolation is important for many evolutionary processes, notice that it would do nothing to help cooperation if the more defection-prone elements were advantaged in each neighborhood. Some individuals would still be surrounded by fortuitous clusters of cooperators, of course, but then it would be the least cooperative individuals in such neighborhoods who would be the most likely to survive, and the societywide process would be ratcheted *downward*. The peculiar strength of the mean does not help cooperation as much as were the most cooperative elements somehow favored, but it does prevent the decline that would happen under more pessimistic circumstances—and, most important, it sets the stage for the eventual convergence of the societywide mean to the mean of such initially rare cooperative neighborhoods.

There are, of course, reservations. First, like those of any simulation, our findings are ceteris paribus; we simply do not know whether the heuristic will work in the "buzzing, blooming confusion" of the natural world in the same way that it does in our simulations. Certainly, we intend no implication that initially uncooperative natural societies will necessarily die out because of the dynamics we identify in our nasty and flat societies. Natural-world societies populated by chronically uncooperative and untrusting people might die out—no doubt, some have—but they might also prosper for reasons unrelated to this heuristic.

More generally, in the natural world we should expect complex interactions—even chaotic effects (see Nowak and May 1993)—among the many variables that influence cooperation. Perhaps the effect of this heuristic will vanish under such processes. Perhaps, alternatively, its effect will be *multiplied* by such complexity beyond anything observed in the laboratory. Our simulations show only the general manner in which the heuristic per se will work. How far the processes they describe might go in natural societies will depend, undoubtedly, on many other factors.

Second, the heuristic by itself cannot have any beneficial effects for cooperation. As we pointed out earlier, absent "other mechanisms" supporting cooperation—at least to some extent—everyone would have a .0 probability of cooperating, and projecting to others' intentions from one's own would lead nobody ever to get involved with anyone else. Nobody would trust anybody, and they would be justified in that expectation. Perhaps it would be appropriate to think about such "other mechanisms" as initial kicks in a positively reinforcing feedback cycle (Maruyama 1963) when both initial kick and the heuristic must be working in order for cooperation to grow.

Notes

We have been helped by, Robyn Dawes, John Dryzek, Fern Linehan, Mark Miller, Matthew Mulford, Timothy Nitz and Langche Zeng.

1. We adopt the term *ecology* in preference to *society* because of the baggage carried by the latter term. As we use it, ecology refers to the interpersonal social context from which the individual's encounters are drawn, nothing more. Ecologies might, of course, be characterized by division of labor, structure, organization, hierarchy, or any of the other features commonly used to describe societies, but ecology is the more basic term, having the advantage for our purposes of referring *simply* to the population that surrounds the acting individual and from which the latter's encounters are drawn. Norton Long's (1958) classic paper "The Local Community as an Ecology of Games" is suggestive in these terms.

2. When p and $1 - p$ are the proportions cooperating and defecting, respectively, in the binary condition and when q and r are the proportions (playing and) cooperating and (playing and) defecting in the trinary condition, then a reasonable estimate of the proportion of *intending cooperators* choosing to opt out is $p - q$ and of *intending defectors* choosing to opt out is $(1 - p) - r$. The proportion of intending cooperators was significantly smaller than the proportion of intending defectors across five separate decisions.

3. If, for example, 70% of a population intends cooperation, then projecting from one's own intentions will be correct for that 70%—conversely, if 70 percent intend defection.

4. This is not to suggest that the heuristic in question is the only means by which we make such estimates; Cosmides (1989), for example, has provided extensive data supporting the proposition that we do have special purpose cognitive mechanisms for detecting cheating in others, and there can be no doubt that we also rely heavily on experience when that is available.

5. This is consistent with Orbell and Dawes 1991 and 1993. If all payoffs are positive, setting aside considerations of *relative* payoff, there is no rational basis for not playing (playing dominates not playing). Alternatively, if all payoffs are negative, then there is no rational basis for playing.

6. For other examples of this recognition that solutions will rarely be perfect, see Trivers 1971 on reciprocity, Frank 1988 on the role of emotions as cue givers about others' intentions, and Buchanan 1965 on ethics.

7. Our simulations include no individuals with a .0 probability of cooperating. Such individuals would never enter and play and so would have no impact on outcomes.

8. The linear relationship between individuals' probability of cooperating and their probability of playing is, of course, only one of many that are possible. But it is the *simplest*, and we adopt if for that reason and also because, absent good theoretical justification for them, alternatives would only add noise, making it harder to recognize patterns in the findings that result from the heuristic per se. Notice that decoupling cooperative propensities and the probability of playing would leave the probability of encountering cooperation- and defection-inclined individuals equal to their proportion in the relevant population with the result that the latter would, in the long haul, come to dominate the ecology.

9. Data are available from the authors on request.

10. A complication of the present model would provide a further source of the requisite diversity via mutation—low frequency, random "errors" in the reproduction of BRC types into subsequent generations. We have not added mutation in the present case because (except with outlandish assumptions about direction, frequency, and magnitude) mutation would not change the thrust of our present findings. Low probability changes that were incremental with respect to the previous generation's BRC and that were not systematically biased in one direction or another would be vanishingly unlikely to change the mean of a whole society and would be quite unlikely to have much effect even within the smaller neighborhood units.

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THICK-SKINNED LIBERALISM: REDEFINING CIVILITY

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A norm of civility *defines a standard of conduct that citizens can rightfully expect from strangers. What are appropriate norms of civility for citizens of liberal states? I argue that two approaches to civility are prominent in our political culture, one requiring "mere" tolerance, the other, that we affirm the worth of others' pursuits (and thereby the worth of those others). This split parallels a division in liberal theory between an interest-based account of liberalism (represented primarily by J. S. Mill) and a status-based account (represented principally by John Rawls and Ronald Dworkin), respectively. The exploration of this theoretical divide and how it relates to contending notions of civility helps to clarify disputes in the broader culture. I conclude that interest-based liberalism offers a more satisfactory approach to the issue at hand.*

There is a ferment in our culture concerning the norms of civility appropriate for liberal citizens. I am not referring to the notion (though it may be true) that as a society we are simply growing more rude. Rather, I am referring to beliefs held by or urged on citizens as to how we should respond to others who hold to conceptions of the good alien—if not antagonistic—to our own. Is it sufficient that we "merely" not interfere or tolerate others as they pursue their own conceptions of value? Or must we somehow do more by affirming the worth of diverse conceptions of the good life that we find not only out of tune with our own but perhaps even trivial or debasing and thereby affirm the worth of those who practice them?[1]

It may be that we are growing less civil while, at the same time, civility is being redefined and its requirements heightened. Those of us on college campuses are particularly aware of this change as we are urged, for example, not merely to accept or tolerate but to "recognize" or even celebrate diversity. Though this edict is frequently related to ethnicity, gender, and/or sexual orientation, we are really being asked to celebrate diverse conceptions of the good grounded in cultures of long or short duration or in a set of avowedly political values. Recognition may involve making available institutional resources including money, office space, and the like and/or creating programs focused on given cultural identities. It may also require not engaging in—even banning—acts of expression that offend given groups (see, e.g., Henry 1993; Hentoff 1994; Hughes 1993).[2]

Often, this implicit redefinition of civility from tolerance to affirmation is grounded in a view of the role citizens play in enhancing or diminishing others' self-esteem by words or actions directed toward the groups with which they identify.[3] Enhancing self-esteem requires that we show through our actions that we affirm the moral worth of given ways of life, whether tied to cultures or to "lifestyle preferences," and thereby the moral worth of the people involved.[4]

These conflicting norms of civility mirror, I believe, an evolving divide in liberal political theory. This divide centers on what it means to treat others with the concern and respect they are owed as persons able to act upon plans advancing a conception of the good life. In particular, to what degree—if at all—does respect for persons entail a respect for the ends they pursue and how must this respect be manifested? And to what degree does concern for the well-being of others oblige us to instruct each other where we can on the worthiness of ends? Put somewhat differently, these questions raise the further issue of what kinds of conversations, if any, about the meaning of the good life are appropriate in the public forums of liberal societies.

I have two main purposes in calling this dichotomy in liberal theory to the fore. First, debates in political theory (where, one hopes, participants are clearer about what they mean than in popular media) can help to illuminate controversies in the political culture at large. I think that this is the case regarding norms of civility. Second, it is important to see that one's core political values have profound implications for how one does, or would if consistent, think about a range of practical problems. One school of liberal thought, of which John Stuart Mill shall be our main example, demands of liberal citizens a full-throated dialogue—even a sharp, impolite dialogue as the occasion demands—regarding the meaning of the good life. This is because concern for others' well-being entails a concern that they use their liberty well, and this in turn obliges us to cajole and persuade—but not coerce—others if we have reason to believe that they are not using it well. The duty to tolerate diverse conceptions of the good as articulated in Mill's harm principle does not entail a duty to affirm the worth of such conceptions. Indeed, to feel oneself compelled toward affirmation and/or withholding of criticism is a failure to show proper concern for others' well-being and hence a failure in one's moral responsibilities. We will explore how Mill is drawn to this conclusion by the fundamentals of his "ideal utilitarian" political thought. Essential to this argument will be to note that Mill grounds rights in interests and takes liberty to be the principle political value because it is so vital to advancing our interest as autonomous (or potentially autonomous) beings.

In contrast, contemporary Kantian liberal theory, which John Rawls and Ronald Dworkin shall represent, grounds rights not in interests but in what I shall call our *status* as persons. In accordance with this view, the fundamental political value is a particular conception of equality, or, in Dworkin's terms, a universal right to be treated with "equal concern and respect." While it may not be entirely intended or essential to the status-based project, its implicit notion of civility suggests that something more than tolerance (let's call it *tolerance-plus*) is morally required. For reasons that are complex and even paradoxical, for the Kantian liberals, respect for persons slides into a respect for ends.[5] I shall argue that one undesirable consequence of this elision is unduly to constrain the conversations that may legitimately take place in liberal societies on the meaning of the good life.

Much of the disagreement between the two views resides in different understandings of how much one's sense of self as an autonomous agent pursuing self-chosen ends depends upon how one thinks he is perceived by others. The essential role others play in enhancing an individual's sense of self-worth (and thus, it is alleged his or her capacity to act in the world) is a much more central notion to the status-based view, though often not with desirable consequences.

I do not intend to—nor can I—resolve the controversy between interest- and status-based liberalism or between those who give pride of place to liberty or equality as the core liberal value. I do want to suggest, however, that we ascertain the acceptability of general theories of liberalism by how satisfactorily they answer concrete questions of political morality. I will suggest that the interest-based view yields a more satisfactory understanding of the moral requirements associated with civility, although as we shall see, the particular interest theory we examine—that of Mill—is open to a host of objections on other scores.[6] So one aim of this essay is to examine a particular and pressing political problem. Another is to contribute a building block to a broader theoretical debate among those with liberal political inclinations.

TWO CONCEPTIONS OF CIVILITY

A *norm of civility* defines the kinds of behavior that persons can rightfully expect from others. By one dictionary definition, it refers to refraining from rudeness, while *politeness* implies some further effort to extend courtesies to others (*Webster's New World Dictionary*, 3d college ed.). And rudeness suggests some degree of intention; that is, the rude person hurts someone's feelings out of malice rather than oversight. *Rightfully expect* implies that persons are duty-bound to be civil toward one another even though incivility is generally best subject to social rather than legal sanction.

Conceptions of civility vary, of course, across cultures. Such a conception appropriate for a liberal culture, while not necessarily higher or lower than other standards (What is the scale of measurement?) does require a fairly high degree of forbearance or tolerance of difference (Orwin 1992). A liberal political philosophy provides reasons for citizens to forbear from actions imposing burdens on their fellows who are pursuing their own conceptions of the good without harming others.

Now liberal societies have met quite imperfectly standards of civility that citizens have reason to accept (and often do accept in the abstract). This fact is the motive behind one of the classics of liberal thought, John Stuart Mill's *On Liberty*. As Mill himself writes, his goal is to provide a principle to guide not only the state but the public as well as it contemplates interfering with the life plans of others. Mill's harm principle, which is in essence a standard of civility, is simply stated; but the reasons behind it are complex and, whatever the criticisms of *On Liberty* and its fit with other works of Mill, reasonably coherent.

On Liberty, though not without philosophical rigor, was intended as a popular work meant to teach two key lessons: (1) the value of liberty in people's own lives and (2) its importance in the lives of others and the social utility that derives from others' use of their freedom. The first lesson is difficult enough as, for Mill, the average person who has not experienced the advantages associated with the exercise of liberty often undervalues that good in his or her own life. If one imagined a game show where one was given the option of trading one's right to free expression for a Winnebago, Mill might suspect that most people would go for the Winnebago. This is in large part because one can only come to appreciate the value of liberty by exercising it, and the many for whom this has never been important are likely to place too little value on it. As someone in the utilitarian tradition, Mill would claim that this undervaluing has deleterious consequences for both individual happiness and collective good.

However negative Mill's assessment of the popular values of his day, he clearly believes that his readers can be brought to recognize the value of liberty in their own lives if they can be made to think about it. Thus in the *Subjection of Women* he writes that the more fundamental problem of practical morality is to get people to extend to others what they know to be true in their own case: "He who would rightly appreciate the worth of personal independence as an element of happiness should consider the value he himself places on it as an ingredient of his own. There is no subject on which there is a greater habitual difference of judgement between a man judging for himself, and the same man judging for other people" (quoted in Gray 1983, 117). In *On Liberty*, he seems to believe that people might make this step for the more purely prudential reason of recognizing the advantages the untrammeled liberty of geniuses, principally in the arts and science, have brought to their lives (Mill 1972a, 132).

Mill's goal is to offer reasons for a standard of civility that he sees as being very imperfectly met in his society and destined to be unmet in mass democratic societies generally. The standard covers not only state interference but private meddling into activities that do not primarily concern or affect the interests of others. These reasons are grounded in an ideal utilitarian theory claiming that the perfection of our rational, aesthetic, and moral faculties brings the most happiness to "man as a progressive being." Since these faculties are only exercised through free choice, one ought to weigh liberty very highly in one's utility calculus. Mill then appeals to utilitarian impartiality between one's own good and that of others in order to see that the good we value for ourselves is as important for them. This should be sufficient reason for one even mildly moved by concern for others' happiness, as we should be, to refrain from interference with their liberty. Fearing that it might not suffice, Mill brings to bear the additional benefits people receive from the self-development of geniuses who enhance human well-being through their labors. The benefits one might receive from the use of liberty accrue only if it is used in certain ways. Mill takes a broadly pluralistic view on the worthiness of ends one might pursue. Indeed, there is some worth in an end simply being *my own* whatever its intrinsic merits. Nonetheless, Mill is often disdainful of the ways in which people actually (mis)use their liberty.[7] And this raises two problems for the conscientious person who comes to adopt Mill's standard. First, how does one respond in the conduct of one's own life to conformist pressures from those who undervalue liberty? Second, how does one deal with those who truly believe that pushpin is as good as poetry (i.e., for whom freedom yields anything but self-development)? Mill responds to both questions in decidedly moral terms, referring throughout to virtues and vices that promote or inhibit the free exercise of one's faculties.

"A person," Mill writes, "whose desires and impulses are his own—are the expressions of his own nature, as it has been developed and modified by his own culture—is said to have a character" (1972a, 128).[8] Now, one evidence of character is resistance to pressures to submerge one's individuality for the sake of popular acceptance: "To be restrained in things not affecting [others'] good, by their mere displeasure, develops nothing valuable, except such force of character as may unfold itself in resisting the restraint" (p. 131). Elsewhere, Mill refers to persons who allow themselves to be forced into a mold formed by popular taste as guilty of "timidity" and, in essence, lacking in character: "If they are of strong character, . . . they become a mark for society which has not succeeded in reducing them to commonplace" (p. 133). Mill defines character in large measure as a willingness to resist popular pressures. The person of character understands him- or herself as someone undoubtedly shaped by cultural forces but whose autonomy is a process of, in Nietzschean language, "becoming what one is." Once one's character begins to take shape, one's sense of self resides only to a small degree in "positive feedback" from others. This again is Mill's message when he claims that "eccentricity has always abounded where strength of character has abounded" and that the amount of eccentricity in a society is proportional to the "moral courage" found there (p. 135).

How does the person of character respond to those who fritter away the opportunities for self-development that liberty of thought and action offers? Let us first note that Mill takes pains to point out that the standard of civility he expounds is "not one of selfish indifference" (ibid., p. 144). We all should have a strong regulative desire to promote the good of our fellows. Indeed, we "owe to each other help to distinguish better from worse, and encouragement to choose the former over the latter" (ibid.). Yet there are presumably times when our altruistic inclinations must be suppressed as we see people pursue the worse, rather than the better, in nonharmful ways. It is here where the harm principle seems to come up against the utilitarian edict to act so as to maximize happiness. There are cases where paternalistic actions (e.g., the prevention of self-destructive behaviors like smoking) will do more good than refraining from such actions. It is thus unclear why a utilitarian would treat the harm principle as anything more than a rule of thumb or something that tends to advance happiness in most instances.

Putting this confusion aside, it is quite clear that Mill relies on some rough ranking of life plans whereby some, even if not directly leading to harm to others, are necessarily not worthy of respect and may naturally engender distaste and even contempt—for example, life plans that place the animal pleasures above or on a par with those of thought and feeling.[9] Mill believes, in fact, that those able to distinguish higher from lower pleasures owe to others their persuasive powers to encourage them likewise to see the distinction and to encourage them to act on it (though not by restricting their liberty).[10] He argues, "It would be well, indeed, if this good office were much more freely rendered than the common notions of politeness at present permit, and if one person could honestly point out to another that he thinks him in fault, without being considered unmannerly or presuming" (ibid. p. 145). We have a duty to act upon our unfavorable opinion of those who choose frivolous or worse life plans for the good utilitarian reason that doing so may, in the long run, promote their well-being. Yet we also have a right to do so "not to the oppression of [someone's] individuality, but in the exercise of ours" (p. 146).

While we might debate Mill's consistency or how well he refutes the charge of indifference, this much seems clear. The goal of the harm principle is to carve out a rather wide sphere within which people can exercise their liberty of thought, expression and action with little thought as to how this concerns others to whom they have no "assignable obligation" (ibid., p. 149). Beyond this, Mill urges upon us a thick-skinned liberalism. We ought to suppress our inclination to castigate those who engage in unconventional conduct we may find offensive yet we should also be bolder in criticizing ways of life that are frivolous or demeaning. Mill's notion of civility might lead to a less "polite" society than he lived in and for us, a society that is impolite in different ways. Finally, we should also conduct our lives with dignity and self-assurance in the face of illegitimate interference from others. This capacity, more than that of leaving others alone, is a mark of character. Both require that we show appropriate self-restraint, and it is far more likely that we will do so if we fully understand the value of liberty—ours and others'—in a well-lived life.

STATUS-BASED CIVILITY: KANTIAN VIEWS

A case will be made that status-based theories of liberalism with widest currency today yield a different notion of civility than does Mill's liberal perfectionism. We show this first by tracing a line of argument in Rawls that leads somewhat circuitously from a claimed central psychological need of persons, that of self-esteem or self-respect, to what amounts to an understanding of civility. We will conclude by then tracing both Rawls's and Mill's notions of civility back to the central premises of their theories of liberalism in order to better evaluate these theories.

Rawls places self-respect among the primary goods that rational agents will seek to maximize from the standpoint of the original position. Indeed, he contends that it is "perhaps the main primary good" as "it provides a secure sense of our own value, a firm conviction that our determinate conception of the good is worth carrying out" (1971, 544; idem 1993, 318). Without it, "nothing may seem worth doing, and if some things have value for us, we lack the will to pursue them" (Rawls 1993, 318).[11] A key reason for accepting justice as fairness is that it supports self-esteem (a term he uses interchangeably with self-respect) to a higher degree than other theories of justice. Moreover, the plurality of associations and groups spawned in a society dedicated to justice as fairness makes it likely that we will find "our person and deeds appreciated and confirmed by others who are likewise esteemed and their association enjoyed" (Rawls 1971, 440). Presumably, the will and confidence to formulate and act on a life plan is critically affected (perhaps even determined) by the response of a like-minded community and, to a lesser degree by the broader society as well.

Rawls claims that his account, showing self-respect to be perhaps the main primary good, "has stressed the great significance of how we think others value us" (1971, 544). Indeed, he believes that if our conceptions of the good are not confirmed by others, we may well be sapped of the will to pursue them. This appears to be an empirical proposition regarding the necessary social conditions for "willing," rather than, strictly speaking, a moral or philosophical claim. It is essentially a prediction of how people will behave when confronted with certain kinds of social pressures. Yet it is rich with moral implications and, as we shall see, it is no mere coincidence that Rawls adopts this stance toward motivational psychology.

For Rawls, the ethical necessity of showing respect for persons generates a need to show respect for the ends persons pursue. It is not clear that this elision is intended—indeed, there is reason to believe that it is not—but it does seem unavoidable. What would it mean, after all, to respect someone while thinking their life goals worthless or deleterious to their own good or that of others? One might say that we should respect persons as bearers of certain capacities, that is, as beings capable of acting on determinate conceptions of the good. But there is a difference between recognizing a potentiality and respecting a person. To respect *actual* persons cannot merely be to respect the abstract natures of their personhood but to respect particular identities and senses of self.[12] And, as much as Rawls has been criticized for inattention to the particular identities and attachments of socially constituted selves, he gives evidence of being very much aware of the need to express respect for particular persons and not just a general idea of personhood. And the particularity of persons is inevitably expressed through their sense of value and through their actions in light of their values, aims, and interests.[13]

This awareness on Rawls's part is perhaps why he claims that under justice as fairness, "we are supposed to *respect one another's ends* and adjudicate their claims in ways that support their self-esteem" (Rawls 1971, 442; emphasis added). Indeed, such respect for ends is vital to our belief that our endeavors are worth pursuing. Thus Rawls draws the specific connection between respect for persons, self-respect, and respect for their ends.

> It is clearly rational for men to secure their self-respect. A sense of their own worth is necessary if they are to pursue their conception of the good with zest and to delight in its fulfillment. Self-respect is not so much a part of any rational plan of life as the sense that one's plan is worth carrying out. Now our self-respect normally depends upon the respect of others. Unless we feel that *our endeavors are honored by them*, it is difficult if not impossible for us to maintain our conviction that our ends are worth advancing" (ibid., 178; emphasis added).

Now let us ask Rawls a question that we posed to Mill. What if the ends that persons choose to pursue, even with suitable social reinforcement, are unworthy of respect? In particular, does the moral desirability of enhancing self-esteem necessitate a respect for ends I may consider trivial, debasing, or simply wrongheaded? Mill's answer to this question was quite clear. Rawls's is not, though he shares with Mill the belief that humans will tend to find greatest satisfaction in ends that tend to realize their higher intellectual, moral, and aesthetic capacities.[14] Despite this hope, Rawls concedes that his conception of the good is so thin as to include counting blades of grass if such is "given a prominent place" in a person's life plan. Rawls uses this as an extreme example and one he would be surprised to find in real life. However, a quick perusal of daytime television would lead one to realize that the example, while showing a lack of lurid imagination, is hardly the most banal or distasteful life plan persons in liberal societies actually choose to pursue.

Before answering our question, let us add one more consideration. We already know from Rawls's theory of justice that "a standard of perfection" has been rejected as a proper political principle, so that "for the purposes of justice [one must] avoid any assessment of the relative value of one another's way of life" (Rawls 1971, 442). Or, as he puts it elsewhere, from the standpoint of justice, "all conceptions of the good are considered equally worthy" (idem 1982, 172). Now, to say this is so from the standpoint of justice is simply to hold that no one should be denied liberty because their (nonharmful) conception of the good is deemed unworthy. This is a position that Mill endorses as well. But Rawls's argument from self-esteem and the virtually infinite latitude he appears to take in defining *what counts* as a conception of the good add constraints to dialogue between holders of such conceptions.

For Rawls, a person's good consists of "ends and activities that have a major place in rational plans" (Rawls 1971, 432). Yet we assess the *rationality* of the plan in terms of whether or not it orders actions to attain the good. A plan is rational insofar as it enables its holder to maximize attainment of enjoyable ends and activities and irrational insofar as it fails to do so. So the good is what persons rationally pursue, while rationality is defined in terms of advancing the good. What this amounts to is that whatever persons most purposefully pursue is their good.

There is a certain circularity in this view but, leaving that aside, it is clear that Rawls offers what he might call thin theories both of the good and of rationality. His view is compatible with the view of the good of pre-Millian utilitarians, as is made clear in his blade-counting example. What ultimately defines a life built around this activity (e.g., ordering other activities to leave maximum "quality time" for it) is that it is the counter's only "pleasure," or it is the counter's "nature to enjoy this activity and not to enjoy any other" (ibid.). To be sure, this is not quite a mindless activity, as the counter is intrigued by the mathematical properties he finds in grassy areas. Nonetheless, a "conception of the good" appears to be equivalent to any set of preferences that are purposefully pursued to bring enjoyment within the constraints of justice. The "Aristotelian principle" (which bears some resemblance to Mill's understanding of the difference between higher and lower pleasures), which tells us that people prefer to pursue complex and challenging tasks, is but a rule of thumb—again, an empirical proposition about what people tend to enjoy—and is not essential to the definition of the good itself (ibid., 433).

Where does this leave us? If respect for persons necessitates respect for ends, if justice requires an official indifference to ends within its constraints, if self-esteem requires an affirmation by others regarding one's worthiness through one's ends, and if self-esteem is "perhaps the main primary good" essential to the capacity to *will* a plan of life, then there seems to be little room for the sort of "impolite" critique of ends that Mill finds essential to the exercise of liberty. Moreover, the less value one places on aiding others (as a mark of concern or respect) to lead a good life, the more the satisfaction with the life one actually leads takes priority in assessing well-being. Hence, psychological dispositions to act on any conception of the good is politically important, while the particulars of that conception, so long as they are within the constraints of justice, are not. The spirit of Rawls's argument leads to this conclusion even if the letter is ambiguous. Thus, in contrast to Mill, it is hard to see the basis for "impolite" dialogue concerning the good life given the thin theory of the good that is offered and the injunction to respect ends that lie within the constraints of justice. Rawls is remarkably nonjudgmental not only about the content of the good but also about the content of one's character.[15] Unlike for Mill, the capacity to *will* a life plan is conceived more as a psychological disposition than a mark of character. Indeed, it is not clear that Rawls sees people as responsible for their own characters so even when this term is used by him, it introduces ambiguities (as in, e.g., Rawls 1993, 76–77).[16] This is perhaps why Rawls is much more likely than Mill to emphasize the social and psychological conditions of willing and to define the good in strictly subjectivist terms. These considerations form the textual basis for claiming that a Rawlsian notion of civility extends beyond toleration and adds an element of affirmation to it.

STATUS- AND INTEREST-BASED CIVILITY: EXPLAINING THE DIFFERENCES

The differences in norms of civility between Mill and Rawls have roots in the fundamental structures of their theories of liberalism. This argument has been implicit in the preceding section. Now I wish to make it explicit by reintroducing notions of interest- and status-based liberalism. I conclude by contending that the interest-based theory leads to a more morally satisfactory account of civility in its capacity to account for a wider array of morally important aspects of the relations among strangers in a liberal democratic culture.

Mill, in essence, grounds rights in interests. Persons have a right to liberty because of the overriding interest they have in leading free lives. The interest is of sufficient weight to impose a duty on others to respect it (see also Raz 1986, chap. 7). Indeed, it is of virtually infinite weight as the right to liberty trumps all sorts and depths of unhappiness which others may experience resulting from its exercise. As with any theory of rights, Mill specifies the ways in which persons must be treated. But, he grounds this specification in a theory ultimately rooted in a notion of the agent's good.

Mill's account of civility, I have suggested, emerges initially from considering the value of liberty in one's own life. Then we are asked to imagine its similar value to others and, in light of this, to refrain from interfering in other's liberty either through the state or through emulating the infamous Mrs. Grundy of Victorian society. Ultimately, civility is defended because of the interest all have in leading lives aimed at their own development and perfection.

A hallmark of Kantian liberalism is a rejection of just such interest-based defenses of liberal principles. The theory that "rights are necessary to protect enduring and important interests fixed by human nature and fundamental to human development, like interests in the choice of sexual partners and choice of religious conviction" is described as familiar but "inadequate" (Dworkin 1985, 369). Dworkin does not spell out his reasons for this conclusion, but they seem to reside in the considerations that the language of interests, even fundamentally moral ones, invokes notions of balancing and of a kind of calculus, if not necessarily a utilitarian one, regarding how best to promote them. Mill, to be sure, must be guilty of a certain degree of wishful thinking in believing that the interest in self-development that he regards as fundamental is always best promoted by the right to liberty he asserts. Developing an interest in autonomy may, on occasion, best be accomplished through paternalistic intervention in an individual's affairs. Further, it may be that self-development as a societal goal is best promoted by granting more resources to those who are most likely to use them wisely in pursuing their own perfection. It may entail, that is, treating some people as means to societal ends.

Dworkin wants to avoid the language of interest calculus which, even had Mill been more consistent and less optimistic, could lead to profoundly inegalitarian arguments. Thus he argues that equal respect owed to citizens is in some sense logically prior to advancing their particular interests, however widespread these may be. It derives from their status as free, equal, rational beings. This is an approach that grounds a right to liberty not in the good of the agent but in what I will call the agent's *status* as a free, equal, rational being. Kantian political morality as best exemplified in contemporary thought by Rawls, and Dworkin ground theories of justice in this deep egalitarian premise.[17] For such liberals, treating persons as equals—in Dworkin's terminology, as entitled to "equal concern and respect"—is the defining duty of the liberal state. The essence of this view is that government "must impose no sacrifice or constraint on any citizen in virtue of an argument that the citizen could not accept without abandoning his *sense of equal worth*" (Dworkin 1985, 205; emphasis added).

I do not want to enter too deeply into the question whether a Kantian or some interest-based account of liberalism is ultimately more successful or the just-as-thorny issue of whether liberty or equality is the defining liberal value. These questions are at the heart of current debates in liberal theory and need more attention than can be given here. I do want to suggest, however, that one's view on these questions necessarily colors one's view on a wide range of concrete issues, not the least being the appropriate norms of civility. Two aspects of status-based liberalism are pertinent here. First, its primary value is a relational one. Equality defines oneself in the first instance in relation to others. To be of equal status is to be on a par with—no better and no worse than—someone else in terms of some quality that two or more persons have in common. Now virtually all of us—certainly all self-described liberals—will grant equality a significant place in our moral universe. Equal protection under law and equal opportunity are perhaps the claims to equal treatment that are backed by the strongest consensus in our society though persuasive cases can be made for equality (or more of it) in many other realms as well. That equality defines one in relation to others is by no means a count against it. Nonetheless, when Dworkin and Rawls introduce a psychological element into their conceptions of equality there is a profound impact on their views of the requirements of civility. When Dworkin mentions one's *sense* of worth (as opposed to one's worth) he is referring to a self-perception that is taken to be, in large measure, a reflection of how others view and act toward one. This is what gives self-esteem a moral and political dimension.[18] The relevant question regarding civility is how or whether one can affirm a fellow's sense of equal worth while deeming his or her life projects unworthy. The deep egalitarian and psychological dimensions of status-based liberalism seem, at minimum, to complicate this task.

We can now draw some points of contrast with Mill. Mill's interest-based liberalism, which gives place to liberty as the fundamental value, is not relational, at least not in the same sense as is the Dworkinian view. Mill's key interest in liberty is in what people do with it and how their actions contribute to their development. What ultimately matters are the choices people make and that these choices develop rational and moral faculties. It matters to a much lesser degree, if at all, how people *feel* their lives are going (their level of self-satisfaction and self-esteem) if in fact their lives are not going well by objective or broadly intersubjective criteria. Hence, Mill's famous remark that it is "better to be Socrates dissatisfied than a fool satisfied" (Mill 1972b, 10). If someone's life is not in fact going well, a morally conscientious person is obliged to lend his or her good offices to instructing—but, for liberals, not coercing—others on the good life. This leads to the sharp, even impolite, dialogue regarding the nature of the good in all its plurality as an essential element of life in liberal societies. Concern for persons does not, in this view, demand respect for ends. Moreover, debate about ends is an essential aspect of liberal citizenship. Thus there emerges a sharp distinction between what must be tolerated and what must or should be affirmed.

Moreover, Mill's very understanding of character leaves little room or need for the kind of validation in the eyes of others as a basis of self-esteem that we see in Dworkin or Rawls. It stands in marked contrast to the status-based views of Rawls and Dworkin, which appeal fundamentally to "senses" of the self as affirmed by others and of character itself as an essentially psychological rather than moral disposition.[19] Mill is not Nietzsche, whose "strong, rich temperaments" experience neither shame nor insult because they are so little moved by what others think of them.[20] Yet as much as valuable projects almost always involve the cooperation of like-minded others, what others think about us has—or should have—very little power to diminish the will to act (or, presumably, the self-esteem) of those with reasonably well-developed characters. Indeed, one is morally blameworthy if one's course of action is altered by others' opprobrium.

WHAT KIND OF CIVILITY DO WE WANT?

If it is granted that we have identified two paths to take in assessing our duties toward others with conceptions of the good that differ from our own, we still must ask, What kind of civility do we want? To answer this question, moreover, we must also consider how much and what forms of contestation regarding conceptions of the good life are compatible with liberal norms of mutual respect—and how much of it is prudent. I think it safe to say that our society is deeply divided as to how this question should be answered and that this division is not unlike the theoretical divide discussed in this essay.

Liberal bashing in recent years in both political and academic forums has often revolved around the charge that liberals are insufficiently attentive to the predominant question of classical political thought, namely, How should the good person live? It has seemed to many that liberal tolerance has become something else—a blanket endorsement of "life styles" with little concern for their intrinsic merit. Much of this debate (like the infamous Murphy Brown flap) is overtly partisan and not to be taken too seriously. Nonetheless, liberals who see no need to surrender the "value issue" should recall that there is a forceful voice in the liberal tradition contending that debates about such questions are the essence of liberal politics. Indeed, not to engage in such debates, particularly when one believes that others are misusing their freedom and thereby depriving themselves of opportunities to lead richer lives, is to be guilty of a kind of moral indifference. To engage in such dialogue is in essence a means of showing an appropriate concern for others' well-being.

What makes this view plausible is the proposition that what gives liberty value is that it advances the good and does so only if used in certain ways. This is the interest-based view that Rawls and Dworkin reject as it may violate a right to equal treatment. Yet it is perhaps worth noting that there is a certain ambiguity in Dworkin's dicta that persons be treated with equal concern *and* respect. There are occasions where meeting one condition makes it impossible to meet the other. Thus I might show respect for you by refraining from comments or actions that you might perceive as an affront to your dignity. For example, I might see you drinking an alcoholic beverage against doctor's orders but refrain from preventing or even discouraging you in order to avoid treating you as a child or as someone incapable of making independent life choices. Concern, on the other hand, could well impel me to discourage you (perhaps call the party host in advance to arrange that no alcohol is served) even if this entails some deception and meddling that is not consistent with showing you respect. In general, concern might require an active intervention in the affairs of others to promote their welfare, while respect may require us to refrain from intervening (though this is one way among many that respect is shown).

Millian civility is better able to pay homage to the ideal that a liberal society ought to be characterized by mutual concern. This should not surprise, because concern bears a direct relation to well-being, and the advancement of well-being is the core value of interest-based liberalism.[21] Mill also preserves an inexorable tension between meeting norms of concern and respect and maintains, if perhaps not fully consistently, that concern cannot justify intolerance.[22] "Liberal citizens who acquire the capacity to sympathize with widely divergent ways of life," Stephen Macedo writes, "acquire a range of 'live options' and an openness to change" (1992, 214). Mill would certainly share this view; he, too, calls for something more than tolerance. We ought to see others in all the diversity of their views and practices, not only as sources of legitimate moral claims to noninterference but also as our educators. We learn from and enrich each others' lives in ways that invite rigorous self-examination and the fullest exercise of personal autonomy.

Robert Frost was being a bit unkind in characterizing a liberal as someone who cannot take his own side in an argument. But there is something to the more moderate view that an initial liberal response to an alien voice is, "There may be something in what you say." The point for Mill, however, is that this is *only* an initial response, not a final word. Respect for others does not require that we affirm their ends or buck up their self-esteem, only that we take them seriously and recognize that they may have something to teach us. At some further point in the conversation, disgust, revulsion—indeed, a full range of emotions associated with moral disapprobation—may be in order. If we are wrongly subjected to expressions of such emotions, Mill urges us to be thick-skinned enough to carry on our plan of life against popular pressures. To fail to do so is a moral failure of timidity, not simply an understandable psychological response. Yet it is also a failure of timidity not to lend our wisest counsel to others who are too ready to conform or to challenge themselves to lead more satisfactory lives. For Mill, there are many paths that lead to such lives, but not all paths do so. His is not a search for a summum bonnum but for reasonable limits built around some rough consensus as to what components a good life might have.[23] It is a question worth pursuing. I believe it is unduly foreclosed in the name of promoting self-esteem and a misguided notion of equal respect by the notion of civility encouraged by status-based liberalism.

Notes

1. It is true that citizens in liberal societies too often fail to meet even minimal standards of tolerance, a matter I shall address. However, my prime task is to explore what a standard of civility should be even if it is and will likely remain imperfectly met.

2. I mention the diversity debate in universities to illustrate with a familiar example a trend toward associating respect for persons with an affirmation of their ends and away from judging the worthiness of the ends themselves. For the best recent discussion of the "politics of equal respect" as it bears on issues of multiculturalism, see Taylor 1992, esp. 63–73.

3. This tendency has been aided by a focus on self-esteem and the need to promote it as a cure for a variety of social ills. Perhaps the most publicized effort in the self-esteem movement was the formation of a California task force to study ways to promote self-esteem, a pet project of state assemblyman John Vasconcellos. See, e.g., California Task Force 1990. For a critical discussion, see Burr and Christensen 1992 and n. 11.

4. In other instances, affirmation or recognition may entail simply refraining from judgments involving such notions as normalcy or deviance (Krauthammer 1993; Moynihan 1993).

5. The paradox resides in the fact that for Kantians, respect owed to persons qua persons resides in their *potentiality* to form and act on ideas of the good, rather than on their particularity (Taylor 1992, 43). It rests, in other words, in something of an abstraction, as when we say that all people possess an inherent dignity, even criminals, scoundrels, and fools. It would seem that Kantians would not be prone to the elision discussed in the text. This is not the case, however, for reasons I shall discuss.

6. Thus a better answer to any important question of political morality like civility is clearly a reason in favor of one type of deep theory over another. But a notion of civility is not the only desideratum by which we evaluate fundamental theories of liberalism. A better answer regarding civility does not resolve the deeper theoretical dispute simply because things other than civility also matter, not the least being conceptions of distributive justice and basic liberties.

7. This attitude is evident in *On Liberty*, though Mill's most extended efforts at distinguishing among ends is found in the essay *Utilitarianism*, specifically the distinction between higher and lower pleasures (see Mill 1972a, 1972b).

8. Here Mill uses "character" to mean a morally exemplary person. He is not always consistent in this usage, as when he refers to the "pinched and hidebound type of human character" he sees Victorian society as encouraging (1972a, 130). This ambiguity is common in everyday speech when we speak of a good person as a "person of character" and someone else as a "bad character." Both refer to a moral standard, yet the first likens this standard to character as such while the latter uses the term neutrally, leaving it to an adjective to supply content.

9. See Mill 1972b, esp. 8–12.

10. Like many others, I have raised the issue of the consistency of this last provision with utilitarianism. Suffice it to say that there are problems. It is not clear that liberty should or does rank so highly in each person's utility calculus or that paternalism can always be ruled out as the best means to promote either liberty or happiness. See Gray 1983 for a defense of Mill that challenges the common charge of incoherence.

11. While this assertion is intuitively plausible, it is an empirical proposition on the relation between self-esteem and motivation; and there is no evidence presented as to its truth. It is as plausible to assume that those with low self-esteem are driven to act in order to prove themselves in the world. Such a view is captured in such commonly encountered terms as the "Napoleon complex" or "overachiever." As best as I have been able to ascertain, the vast empirical literature on the relation of self-esteem and a host of other psychological variables has yielded few firm findings. This is at least the conclusion of two recent essays that have reviewed more than fifteen hundred psychological studies on self-esteem. The most recent thorough study concludes that the "news most consistently reported . . . is that the associations between self-esteem and its expected consequences are mixed, insignificant, or absent" (quoted in Burr and Christensen 1992, 461).

12. Charles Taylor comes to a similar conclusion in a slightly different context. The "demand for equal recognition," he writes, "extends beyond an acknowledgement of the equal value of all human beings potentially, and comes to include the equal value of what they have made of this potential in fact" (1992, 42–43).

13. This mildly communitarian reading of Rawls may be controversial. Before giving direct evidence, let me suggest that it seems consistent with one general goal of Rawls's work of providing empirical content for notions that remain entirely transcendental in Kant's own ethical writings. Thus general information about human psychology and general social dynamics are allowed into the original position, so that "no longer are [Kantian moral] notions purely transcendental and lacking explicable connections with human conduct"; And justice as fairness does not deal with justice among Kant's purely noumenal selves but "is a theory of human justice and among its premises are the elementary facts about persons and their place in nature" (Rawls 171, 256–57).

14. Rawls (1971, sec. 65) refers to this inclination as the Aristotelian principle.

15. I challenge elsewhere Rawls' and Dworkin's understanding of the term *conception of the good life*, arguing in essence that it is so broad and vague as to be unable to distinguish between an abiding conception of what gives value to one's life and any set of raw preferences, suggesting that there are better ways to formulate the notion without running afoul of the moral egalitarianism that I share with those authors (Sinopoli 1993, esp. 653–57).

16. Rawls's understanding of character, which essentially severs it from notions of desert, has been widely commented on, particularly by communitarian critics such as Sandel (1982, esp. chap. 2).

17. I say "deep" so as to avoid confusion with Rawls's lexical ordering of principles. He argues that liberty cannot be sacrificed to promote greater economic equality but that what legitimizes the two principles initially is that they would be chosen from an initial standpoint of equality that rules out bargaining or threat advantage. And Dworkin argues in a similar vein that fundamental rights function as trump cards protecting individuals from unequal treatment. "The ultimate justification of these rights," he claims, "is that they are necessary to protect equal concern and respect" (Dworkin 1985, 198). See also Dworkin 1978, where he proposes that "individual rights to distinct liberties must be recognized only

when the fundamental right to treatment as an equal can be shown to require these rights" (p. 274).

18. Bruce Ackerman is another prominent Kantian liberal for whom the status-based account of liberalism dictates that no exercise of state power is legitimate if the reason a power holder offers for its exercise asserts that "his conception of the good is better than that asserted by his fellow citizens" (1980, 11). Such a claim, no matter what the possible reasons for it, is an affront to the sense of equal worth of persons.

19. I recognize, of course, that these are not opposites and that one can reasonably speak of psychological motives for moral actions.

20. In "The Genealogy of Morals," Nietzsche offers Mirabeau as a modern example, "who lacked memory for insults and meanness done him, and who was unable to forgive because he had forgotten" (1956, 173).

21. It is perhaps worth pointing out that the arguments of "interest-based liberals" need not take utilitarian form. What is essential is that rights be grounded in some fundamental notion of the human good. For nonutilitarian versions of this view, see Raz 1986 and Galston 1991.

22. And there are Millian voices in our culture wars as well even if they are not the predominant ones at this moment. One such is Andrew Sullivan (1993), whose thoughtful piece on the politics of homosexuality urges what he calls a liberal solution: "This liberal politics affirms a simple and limited criterion: that all *public* . . . discrimination against homosexuals be ended and that every right and responsibility that heterosexuals enjoy by virtue of the state be extended to those who grow up different. And that is all. No cures or re-educations; no wrenching civil litigation; no political imposition of tolerance; merely a political attempt to enshrine formal civil equality in the hope that eventually, the private sphere will reflect this public civility" (p. 36). Sullivan concludes with a note of pessimism regarding the appeal of his liberal politics to either gays or straights.

23. Nor, of course, should our search be for a summum bonum. The reader has no doubt noted that I do not try in this article to articulate or defend given conceptions of the good but only to consider what kinds of questions regarding such conceptions may be in order in liberal societies.

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MARY ASTELL (1666–1731), CRITIC OF LOCKE

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In the now considerable literature reevaluating the reception of Locke's Two Treatises, *no mention has been made of perhaps his first systematic critic, the commissioned Tory political pamphleteer, Mary Astell. Contemporaneous with Charles Leslie, who is usually credited with the honor, Astell had diagnosed Locke's political argument by 1705 and perhaps as early as 1700. Why has her contribution remained unacknowledged for so long? It is argued here that for too long commentators have been looking for the wrong person in the wrong place. Astell correctly saw that Locke's political philosophy was inextricable from his psychological and theological systems, addressing all three in works that were political, theological and homiletic. But why Locke, and why in 1700–1705? Did Astell already know the authorship of the* Two Treatises, *only officially established in 1704 with the publication of the codicil to Locke's will?*

The relative absence of texts in the history of political thought on and by women reflects not women's political or literary incapacity but rather the separate spheres that the social construction of gender imposed. The public and private worlds into which social life was divided and which it was the intention of the theoreticians of the early modern state to entrench were founded on homologous contracts: the social contract, which constituted the political world of men, and the marriage contract, which governed the private world of women. Female (let alone feminist) texts in political thought were an anomaly, given that women were until the twentieth century officially not political creatures. Yet in rare cases, like that of Mary Astell, we have them. The status of married women as legal minors in Western Europe and North America until enactment of married women's property acts of the 1880s barred their participation in states in which political rights were tied to the capacity to own property. Astell's devastating critique of the marriage contract/social contract analogue, her critique of John Locke, (perhaps the first systematic analysis of his entire corpus) and her exposure of the theories of Locke and Hobbes as fathers of a liberalism that did not extent to women anticipated, to a surprising degree, modern feminist critiques of a liberal democratic state as yet unborn.

Astell was a complex figure. As a High Church Tory pamphleteer, interlocutor with Cambridge Platonists, critic of the Dissenters, the Whigs, and Locke, she was equally revered and reviled by prominent men and women in her day. A north country gentlewoman whose Royalist family through death and debt had fallen upon hard times, she had departed her native Newcastle-upon-Tyne for London at around the age of 20. She immediately introduced herself to Anglican circles by presenting a book of juvenile religious poems to the former Archbishop of Canterbury and subsequent nonjuror William Sancroft, who may have provided her financial support; entering a circle of like-minded intellectual women and High Church prelates and their wives; and eventually settling in the house of Lady Catherine Jones, to whom two of her works are dedicated. Astell thus earned her credentials as beneficiary of a feudal patronage system that admitted noblewomen and gentlewomen of talent to limited participation in the political sphere from which they were officially excluded. But then we are perhaps too ready to accept the public/private distinction that the Whig view of history imposes. Astell, Aphra Behn, and Mary de la Riviere Manley—politically active and, indeed, Tory publicists—gave the lie to the very spatial distinctions Locke and the Whigs were concerned to enstate as women nibbling at the margins of politics had done for centuries. Despite rhetoric that belied her considered commitments, Astell remained a true daughter of the Church of England and a supporter of the institutions of monarchy and marriage. Her challenge to Locke to extend to women against domestic tyrants the liberty he claimed for subjects against the Crown was a subversive stratagem. Its object was to expose to ridicule the tenets of contractarian liberalism and certainly not to endorse the space they appeared to open up. She challenged the division into spheres (a public realm of freedom over against which the private confinement of the family was set) on which Lockean liberalism was predicated. In this respect she dared Locke at least to follow Hobbes, who, having ousted patriarchalism from the public space, was unwilling to readmit the Aristotelian *despotes* in the private sphere. Her rationalism, more ruthless than that of her adversaries, impressed on her the contingency of all social institutions. Reason could no more empower citizens to authorize or delegitimize a sovereign than it could empower husbands as monarchs in their families or enfranchise women to overthrow them. Relying on age-old Scholastic distinctions between the authorization and delegation of power, she believed that only God could authorize, even if it fell to "men" to designate an incumbent. Such was the kingdom of this world; only in the hereafter, as deathless and sexless "angels," would women enjoy the freedom to which they so rightly aspired.

What prompted Astell's extraordinarily prospective critique of Locke, and how can we be sure that he was really her target? Critics may question the extent to which Astell would have been specifically concerned with Hobbes or Locke, who in twentieth-century political theory are too often taken to subsume all the complex views of their age. In Astell's case, however, we can cite special reasons for her interest, in the form of personal antagonisms roused by Locke and Damaris Masham, whom Astell took to be his surrogate, over the publication of letters between Astell and John Norris of Bremerton, as part of the debate centered around Locke's *The Reasonableness of Christianity* of 1695. Not only does she mention Hobbes and Locke by name (the latter on several occasions), but it may have occurred to her that it would do little for Locke's personal standing to class him with the notorious Hobbes.[1]

The story is a curious one. Astell's first published work, *A Serious Proposal to the Ladies for the Advancement of Their True and Greatest Interest*, part 1 (1694), had been initially taken for the work of Masham, as typical of the daughter of Ralph Cudworth (Ballard Ms. 41:133, cited in Smith 1916, 113; Perry 1986, 87). But when in 1695 the Astell–Norris letters were published, it was Masham who hastened to criticize them in her *Discourse Concerning the Love of God* (1696). Norris replied directly to Masham's pamphlet in the fourth volume of his *Practical Discourses* of 1698, assuming it to be the work of Locke, an assumption that Astell also made. But Astell held her fire, responding directly to Masham and her mentor only after Locke's death, with *The Christian Religion as Profess'd by a Daughter of the Church*, of 1705. This, Astell's four-hundred-page magnum opus, was a systematic treatment of the then known works of Locke—the *Essay Concerning Human Understanding* of 1689, *The Reasonableness of Christianity* and *Vindication of the Reasonableness of Christianity* of 1695, and *Two Treatises of Government* of 1690—and some not by Locke, namely, the anonymous *Ladies Religion* (1697) and Masham's *Discourse*.

Astell's *The Christian Religion*, initially attributed by Lord Stanhope to her Chelsea neighbor and acquaintance, the Tory Francis Atterbury was a work long in the making (Smith 1916, 113). Intimations of its arguments are to be found as early as Astell's *Reflections upon Marriage* of 1700, the famous Preface appended to the third edition of this work in 1706, and as late as *Bart'lemy Fair* of 1709—a response to Anthony Ashley Cooper, the third earl of Shaftesbury's *Letter Concerning Enthusiasm*. The latter work by Locke's pupil had been addressed to Lord Somers, Locke's mentor.

Astell's carefully thought-out reply to Locke in *The Christian Religion* diagnosed the central tenets of his doctrine only now established with the discovery of hitherto unpublished texts (Farr and Roberts 1985; Goldie 1978; Schwoerer 1990). They are three. The first is the principle of *self-preservation*, which Locke shares with Hobbes and on the basis of a remarkably similar theory of human psychology. Its target was Scholastic metaphysics. The second is the right of *freedom from hereditable incumberment as regards one's person* but not one's property. Its target was hereditary monarchy. And the third is the old principle of *popular sovereignty*, that the power of government is based on popular consent. Its purpose was to legitimate dynastic change. These principles are systematically under attack in Mary Astell's three Tory pamphlets of 1704, *Moderation Truly Stated, A Fair Way with the Dissenters*, and *An Impartial Enquiry into the Causes of Rebellion and Civil War*.

LOCKE AND THE SETTLEMENT OF 1688

Locke's contribution to the constitutional debate of 1688–89 has recently been reevaluated.[2] For some time it had been minimized, due to the anonymity of the *Two Treatises*, written between 1681 and 1683, published in 1689 but with 1690 on the title page, and only officially acknowledged by Locke in the codicil to his will of 1704. Peter Laslett (1963), in a pioneering work of textual excavation, claimed Locke's *Two Treatises on Government* not to have been a rationalization of the Glorious Revolution, as had hitherto been believed but a work written in the Exclusion Crisis challenging Stuart dynastic rule. James II had so thoroughly discredited himself among Whigs that opposition to his removal was negligible. It was the terms in which he was removed that were fought over. These ranged from Tory theses of James's moral incapacity to rule, evidenced by his tyranny, to loyalist "theses of contract, non-contractarian resistance, possession, conquest and abdication," to Whig political theory with roots in the radical reformation thinkers Calvin, Luther, and Melancthon (Goldie 1978, 76, 82). Among them only two references to Locke's *Two Treatises* are to be found, both by Whig radicals. Samuel Johnson (who was flogged while Locke fled), Algernon Sidney (executed in 1683), and Tyrrell were more famous for Lockean arguments than Locke, who received a brief notice in Le Clerc's *Life* and a notice by Tyrrell but was otherwise unmentioned by name as author of the *Treatises* until 1703 (Goldie 1978, 230–31).

Scholars have shown how the constitutional debate that postdated the Revolution of 1688 rehearsed the arguments of the Engagement Controversy 40 years earlier (Scott 1988; Skinner 1972). Tracts from 1649 were republished and their arguments rerun, in some cases with reference to Hobbes and Grotius but almost never with reference to Locke. The works of Anthony Ascham (1649), Philip Hunton (1643), George Lawson (1660), Henry Parker (1643, 1650) and John Milton (1689, 1690) were republished (Goldie 1978, 79). The arguments of John Goodwin, Richard Baxter (whose *Five Disputations of Church Government* of 1659 was mentioned by Astell in *A Fair Way* [1704a, 214]), and James Owen (author of *Moderation A Virtue* (1703), to which Astell replied) were re-rehearsed. The latter were radicals condemned in the Oxford

Decree of 1683. Once again Locke was absent, at least by name.

If Locke was absent, why was he absent, given the centrality of arguments in the second of the *Two Treatises* to the constitutional issues of 1688, to which, in the preface, they seem to be addressed? Some commentators now question how much the Revolution of 1688 concerned Locke and whether, in fact, there were any revisions necessitated by the delayed publication of the *Treatises*. Richard Ashcraft, who reinstates the view that Locke's *Two Treatises* relate to the Exclusion Crisis of 1679–83, principally, gives an explanation for the secrecy surrounding their authorship:

> The *Two Treatises of Government* reflects the language of Shaftesbury and the Rye House Plot and of Monmouth's Rebellion, and not the language of the Whig and Tory magnates who managed the Glorious Revolution. The individuals most deeply involved in marshaling the social, economic, and political forces necessary to settle the crown on William and Mary were, with very few exceptions, the very men who had prosecuted the Rye House conspirators and who led the army and militia against Monmouth's troops. It is hardly surprising that they preferred not to be reminded of the events and ideas associated with the radicals' activities in the 1680s. It is from this standpoint, I shall argue, that we can understand the reception given to the *Two Treatises*, and the gulf that stands between the arguments of that work and the posture adopted by the defenders of William III's government and by those who tried to create an official Whig ideology in the 1690s. (1986, 579)

But Locke had also joined cause with the Whig magnates involved in the Glorious Revolution, and he too hoped to settle the Crown on William III as a dynastic closure to the perennial threat of popery and arbitrary government under the Stuarts. Is this why he was so reticent about the authorship of the *Two Treatises*? And was it for this reason that commentators, now familiar with the establishment Locke, who had sailed over with Princess Mary, and who had been offered a line of government jobs, hesitated to pin the authorship of such a radical work on such a trimming man? Locke, after his immersion in the conspiracies of the Shaftesbury circle against Charles II in the 1680s, had fled to Holland, inhabiting the periphery of William of Orange's court. Although personally absent from the Revolution of 1688, through the good offices of Viscount Mordaunt who had been a Shaftesbury supporter in the House of Lords, he had found a place on Princess Mary's ship in February 1689 (Schwoerer 1990, 533). If the Whig magnates who had prosecuted the Revolution of 1688 did not want to be reminded of the radicalism of the early eighties, perhaps Locke did not either.

Unnamed and perhaps redrafted, refocused to vindicate William (as the preface suggests), Locke's *Two Treatises* entered a flow of pamphleteering in 1689–93 to the extent of some 200 items, equalled only at other political turning points—in 1642, 1648, 1660, and 1710. Its reception as part of the allegiance controversy had important consequences, whatever Locke's original intentions may have been. While in length and degree of philosophical abstraction, the *Two Treatises* may stand apart from other items in the allegiance debate, they represent a radical Whig tendency that had other adherents. Identifying six types of justification for the Glorious Revolution in the corpus of pamphlets, Goldie then analyzes eight political positions on the Revolution, defined by doctrines and interests of the adherents. Only the radical Whig position, to which Locke's *Treatises* belong, along with the works of Tyrrell, Samuel Johnson, Atwood, Blount, and Defoe (later the Locke popularizer whom Astell debated), "used a natural law case for resistance or right of deposition," although a Whig middle group used contractual resistance in some form (Goldie 1980b, 508–9).

Recent scholars have thus firmly reinstated Locke at the centre of the debates of 1688–89, finding confirmation in rediscovered documents in which Locke is to be found advising contemporary politicians and commenting on constitutional and dynastic issues. These documents, together with evidence that Locke's political advice was sought and that he was offered government posts during the 1690s (Schwoerer 1990, 533), have caused revision of the judgment that Locke's political philosophy was more philosophical than political (Thompson 1976, 191). Locke becomes visible in the politics of the Convention and the Allegiance Oath of 1689—even if, so to speak, behind the arras. In the 1670s–80s he was prompting from behind the chair, as in the case of Shaftesbury's famous *Delenda est Carthago* speech of 5 February 1673 to the Commons, to which Astell alludes. She refers sarcastically to Shaftesbury as "a Leading Peer among the *Whiggs*, and who consequently wou'd be thought a Great Patriot and Friend to Liberty, and very much in the Interest of his Country, [who] took the freedom to say in the House of Lords, *Delenda est Carthago*" (1704b, 22). Locke was perhaps advising Somers behind the scene on the Bill of Rights in the spring of 1689 (Cranston, 1957, 325; Schwoerer, 1990, 543). He was there addressing his minute on the constitutionality of the Allegiance Oath against James II to Clarke, a member of Parliament, some time after March 1690 (Farr and Roberts 1985, 391; Schwoerer 1990, 546). And he was to be found commenting in an unpublished paper on Sherlock's *Case of Allegiance* some time after 1690.

ASTELL'S CRITIQUE OF LOCKE: THE CASE

But still no satisfactory answer has really been given to the question "why it took until 1703 before 'any Tory or Jacobite considered it worth their pains to engage in a critical examination of the Treatises'" (Nelson 1978, 105) or why the first "extensive" critique of the second treatise awaited the anonymous *Essay upon Government* of 1705. The argument is made here that scholars are still looking for the wrong person in the wrong place. Not only did the systematic refutation of Locke not await the works of 1703 and 1705 customarily cited, but reasons lie in the centrality of religion to politics as the great issue on which constitutionality itself was made to turn. It is to the response to Locke's religious works that we must look for answers to the *Two Treatises*, which were correctly treated with his theology as a seamless whole. Here we find not only Leslie, already credited with the first systematic response to the *Two Treatises* but, in works that purport to be educational, theological and homiletic, Mary Astell proves to be more single-mindedly devoted to the refutation of Locke than any of her contemporaries.

It is, moreover, a Locke who until recently has been eclipsed by the Locke of the American Revolution, the Locke of possessive individualism (or of the political economists) and other post-Enlightenment Lockes. Astell's Locke, in fact, provides a test of authenticity that only very recent scholarship has met. For this reason, perhaps, her pioneering critique was for so long overlooked. The issues on which it focuses are Anglicanism, trinitarianism, and the settlement of 1688; contracts, oaths, and political allegiance; biblical patriarchalism and the claims of dynastic monarchy; and the rights of freeborn Englishmen versus the ancient constitution. On religion and the settlement of 1688, Astell negotiates the issues of unitarianism, Socinianism, and dissent, all with reference to Locke. On the question of oaths, contracts, and political allegiances she has no difficulty in seeing Locke's purpose to undermine the traditional sources of hereditary monarchy—something perceived by William III as well, who saw himself hostage to the consent of people in a way that did not please him (Schwoerer 1977, 851–53).

Locke's attack on Filmerian biblical patriarchalism was similarly targeted. *Father-right* Locke could easily show to be the most simple-minded basis for hereditary monarchy. Filmer, whose relentless defence of patriarchal lineage represented a desperate attempt at royalist propaganda on the part of one whose lifeline to the episcopacy and the court had been severed by imprisonment under the parliamentarians and who was willing to defend at any cost a position that might ensure the return of the Stuarts (Sommerville 1991, xi) had become the bastion of post-1688 Tory orthodoxy. But not for Astell, who invoked a long line of powerful biblical women against the proverbial patriarchs. In any case, although Filmer's particular genius in marrying natural law and monarchical theory appealed to a wide range of Royalists, including, most notably, Robert Brady, the Royalist position did not depend on Filmer. Defence in terms of Romans 13 and other biblical quotes that sanctioned local constitutional arrangements, as made by Astell, was typical and deemed sufficient (Ashcraft 1986, 187).

It was the umbilical between patriarchalism and hereditary monarchy that Locke had set out to sever, which he could easily do, given the inherent weaknesses of genetic arguments as opposed to argument from reason (Schochet 1975). On the question of contracts and constitutionalism, he took the high road of reason once again, to bypass the constitutional complexities of legal case and countercase (Thompson 1987, 280–82). The upshot is a doctrine of "natural rights" that lends to his work the much-noted philosophical abstraction, erroneously interpreted by some (idem 1976)[3] as a detachment from politics. Astell, not taken in, makes the doctrine of natural rights the frequent butt of her ridicule. If she was no advocate of the rights of freeborn Englishmen, spurning traditional appeals to the ancient constitution (Pocock 1957), denying the salience of even Magna Carta, she was no supporter of biblical lineage either. To argue from heredity, as such, was to resort to contingency, but Astell defended monarchy on the basis of divine design. As a Cartesian rationalist and Cambridge Platonist she paradoxically participated in a "rationalist" movement that was ultimately to allow Whig encroachments on Tory territory and to erode the very constitutional position she supported.

The relation between contracts, covenants, and the constitution was not a simple one. The constitutional politics of Locke are by no means straightforward either. A supporter of the Stuarts in the 1680s, he promulgated the Allegiance Oath against James II in 1690; a supporter of popular sovereignty during the Convention Parliament of 1689—to which he may have been an informal advisor (Schwoerer 1990, 532–35)—and of the Bill of Rights in 1689, he demanded all citizens take an oath of allegiance to William in 1690. It is generally believed that the anomalies of Locke's constitutional position did not become apparent until subjected to intense scrutiny in the debate over the American colonies in the 1760s (Nelson 1978, 106–7). But these very anomalies undoubtedly account for Locke's own failure to commit himself on constitutional specifics, the significance of his silences having become a matter of recent debate (Thompson 1987). Lois Schwoerer believes that Locke's "praise of 'our ancient government' [in his minute to Clarke] is foreign to his theoretical work, which of course is cast in the non-historical idiom of reason and natural rights" but that he was personally committed to the constitution as comprised of King, Lords, and Commons, the missing portions of the *Two Treatises* might tell us (1990, 540–43). Thompson, while conceding with Franklin that Locke is grappling with the technical problem of resistance in mixed monarchies, believes that he wished to extricate politics from the teeth of constitutional lawyers and for this reason did not commit himself on the details, preferring the "rationalist" Hobbesian and Grotian state-of-nature model of social contract (Thompson 1987, 290–91; see Franklin 1978, 105–8). This was not a contract at all but a peculiar amalgam of social anthropology and ancient history with parallels in the biblical Fall.

Astell, consistent with commentators in her day, cast Locke with Hobbes. Whig state-of-nature arguments similar to Locke's, which emerged during the Exclusion Crisis, were routinely treated as Hobbist by the Tories; and it was only after 1760 that the differences between them became of general interest. Moreover, analysis of her characterization of the Whig position which Locke represents, reveals appeals to the rights of freeborn Englishmen as vaguely expressed generalities that were perhaps deliberately evasive on constitutional technicalities. This was typical of airy references to English liberty by the Country Whig, John Tutchin, by White Kennett, and by Defoe, for whom the phrase becomes a Whig catchcry.[4] Astell intuited that Locke, by his very vagueness, had succeeded in disaggregating the ancient constitution and customary right that a generation of Whigs, including Atwood and Burnet, had tried to preserve by melding contract to conquest—a technically impossible project.

ASTELL'S CRITIQUE OF LOCKE: THE EVIDENCE

What is the evidence for the claim that Locke is Astell's consistent target, given that Lockean arguments mounted by Tyrrell, Blount, and Sidney were more widely known and given that Astell's full critique of Locke did not appear before 1705? To take the explicit references first, Astell's first named reference to Locke, in *Moderation Truly Stated,* is to his theory of the association of ideas: "Then Lukewarmness and Indifferency in our Profession, is the only sense in which *Moderation* can be taken in the present context; if with the *Great* Mr. *Locke,* it be our constant care to annex to the word a determinate Idea" (1704c, 10–11). Her remarks preface a long diatribe on the tactics of the classical Whig—a Low Churchman who puts party above all. To effect the balance, he will throw Presbyterians into the scales—"and if this is not sufficient, we can add *Independents, Anabaptists, Socinians,* and what not, to make a dead weight upon occasion" and deny "the Prince a Prerogative" (p. 12). If these inducements are not enough, she says (perhaps lumping Locke with Hobbes), "whoop, the Government is almost shatter'd to pieces, and we're within a hair's breadth of being once more in a State of Nature!"

Astell's second named reference to Locke occurs in *An Impartial Enquiry* (1704b, 40). Ostensibly a reference to his sensationalist psychology, it again connects his epistemology to his politics, this time with a sarcastic comment on Locke as party to Shaftesbury's manoeuvering against Charles II and "the French threat" during the Exclusion Crisis (Jones 1987, 152–55):

> Only let me recommend to all such Thinkers, Mr. *Lock's* Chapter *of the Association of Ideas*; they need not be afraid to read it, for that ingenious Author is on the right side, and by no means in a *French* Interest! And indeed, till People will observe the excellent Precepts of our Holy Religion, and that in particular, of calling no Man Master upon Earth, of following no Popular Speaker and Leader of a Party, they will easily be persuaded to *think,* as every Cunning and Factious Man will have them. (Astell 1704b, 40)

Astell's reference to John Locke for a Whiggish theory of the relation between ideas and evidence is a stroke of genius. Chapter 33 of volume 2 of Locke's *Essay Concerning Human Understanding,* entitled "Of the Association of Ideas" (sections 4 and 5 were entitled "A Degree of Madness" and "From a Wrong Connexion of Ideas," respectively) was among the chapters added by Locke for the fourth edition of 1700 (Locke 1823a, 2:148–57). Astell notes that Locke was politically correct for White Kennett, one of only four Whig bishops to have preached commemoration sermons on the anniversary of the death of Charles I before Parliament, whose earlier commemoration sermon is the object of her critique. Not only could Locke not be implicated with the French in Kennett's eyes, but, in fact, he was one of those who publicized the French threat.[5] The passage must be read as an elaborate satire of Locke's views. Locke, while professing to "observe the excellent Precepts of our Holy Religion," was a latitudinarian, if not a Deist. While claiming to call "no Man Master upon Earth" and follow "no Popular Speaker and Leader of a Party," he was, of course, the follower of Shaftesbury, who exactly fitted that description. This kind of double talk is facilitated, she seems to be suggesting, by Locke's sensationalist psychology, which easily allows people "to *think,* as every Cunning and Factious Man will have them."

Locke is not Astell's only possible target and some of her strongest invective is reserved for Defoe, widely acknowledged as Locke's popularizer and yet more than just his man (Phillipson 1993). In *A Fair Way with the Dissenters,* Astell remarks that were it not for the Christian injunction to charity, she would vent "all that Contempt that is due to little Scriblers and Busiebodies, who, either for Bread, and to deserve their Wages of the Party, or out of an innate Love to Mischief, alarm the Mob, and impose upon the Ignorant and Careless Reeader [*sic*], by venting bold Slanders and notorious Untruths, in a plausible Stile and with some shews of Probability, with an Insolence peculiar to themselves, and a matchless Effrontery" (1704a, 2–3). Similar accusations later in the same year seem also to be aimed at Defoe, again classed among "those Mercenary Scriblers whom all sober Men condemn, and who only write after the Fact, or in order to it, to make their own Fortunes, or to justifie their own Wickedness" (p. 29). Astell's phraseology is close to that of a work published in the following year by an anonymous Jacobite-nonjuror, *An Essay upon Government: Wherein the Republican Schemes Reviv'd by Mr. Lock, Dr. Blackal, &c are Fairly Consider'd and Refuted* (1705, 59–60), which refers to them as "licentious, mercenary and ignorant scribblers." And in 1704 Defoe and Tutchin had been sued for libel (Goldie 1978, 48–49).

The third set of explicit references to Locke is in *The Christian Religion* sections 139 and 312, where Astell argues against self-preservation with explicit reference to the *Two Treatises* (2.149), claiming:

> 139. Suppose our Enemy is a Persecutor, and does invade that *Fundamental, Sacred and unalterable Law of Self-Preservation* (*Two Treatises of Government*, B.2, S.149.), as some call it. . . . A Persecutor who wou'd deprive us of our dear and desirable Estates, Offices and Employments, our dearer Lives perhaps, (over which we our selves *have no power* (*Treatise of Government*, B.2, S.23.), therefore how shou'd he come by't, tho' we invested him with all the power we had, on condition he kept his compact?). (1705, 133)

And again

> 312. What then is *Self-Preservation*, that Fundamental Law of Nature, as some call it, to which all other laws, Divine as well as Human, are made to do Homage? and how shall it be provided for? Very well; for it does not consist in the Preservation of the Person or Composite, but in preserving the Mind from Evil, the Mind which is truly the Self, and which ought to be secur'd at all hazards. It is this *Self-Preservation* and no other, that is a *Fundamental Sacred and unalterable Law*, as might easily be prov'd were this a proper place; which Law he obeys, and he only, who will do or suffer any thing rather than Sin. *No Man having a power to deliver up this Preservation, or consequently the means of it, to the absolute Will and arbitrary Dominion* [306] *of another*, but *has always a Right to Preserve what he has not a Power to part with* (*Two Treatises of Government*, B.2, S.149.), as a certain Author says in another Case where it will not hold. (ibid., 305–6)

So far, it appears, we have no significant critique of Locke by Astell before 1703, the date of Leslie's critique. In the first edition of *Reflections upon Marriage* which ran to four editions in her lifetime, Astell had made an ironic argument for female slavery and male despotism implicit throughout that work, to be later amplified in the famous 1706 Preface: "How much soever Arbitrary Power may be dislik'd on a Throne, not *Milton* himself wou'd cry up Liberty to poor *Female Slaves*, or plead for the Lawfulness of Resisting a Private Tyranny" (1700, 27).[6] Astell's arguments against this double standard appeared, unreferenced, in the first edition of *Reflections upon Marriage* (1700; 29, 32, 38–41, 92–5); while in *An Impartial Enquiry* (1704b, 10, 12, 17, 19, 31, 54), she attacked the case for self-preservation mounted by Locke and his surrogates. In the latter, Astell cautions White Kennett, alluding unmistakably to what she considers to be Locke's religious apostacy in *The Reasonableness of Christianity*, a constant refrain with her:

> Beware of every one who wou'd draw you into *a necessity* of *believing*, that your *Liberties and Estates are in some danger*, who wou'd give you such a *Prospect*, and work you into such a *Persuasion*, and so draw you in by the old Cant of *Self-Preservation*, tho' they seem to demonstrate ever so great a *necessity* (*Page* 18.): Much more ought you to abhor being *drawn in* by the bare *meaning* of it, at least if you have any regard to real Self-Preservation, and think your Souls of greater moment than your Lives or Estates. (ibid., 10)

This is a paraphrase of the central passage of the *Two Treatises* (sec. 209), Astell's quotation of Locke's words being indicated by italics, in which he asserts the right of resistance, with clear reference to James II and his Catholicism: "But if either these illegal Acts have extended to the Majority of the People; or if the Mischief and Oppression has light only on some few, but in such Cases, as the Precedent, and Consequences seem to threaten all, and they are perswaded in their Consciences, that their Laws, and with them their Estates, Liberties and Lives are in danger, and perhaps their Religion too, how they will be hindered from resisting illegal force, used against them, I cannot tell." (1988, 404–5 and n.).

Astell's case is that the right of conquest, denied by Locke to James, is asserted with respect to the people who are granted the right to resist and place their own candidate on the throne. Locke's subsequent endorsement of William III was to bear out her charge. Commentators early and late have noted that if self-preservation is the motive to contract in Locke's system, tacit consent is too frequently the means, a vehicle too weak to do the job. But Astell's argument is a different—and in some respects a more compelling—one. The weakness of the push of consent against the pull of self-preservation is simply indicative of the sleight-of-hand by means of which de facto conquest is converted into de jure rule. Self-preservation dictates that right give way to might, as Astell percipiently remarked: "And if mere Power gives a Right to Rule, there can be no such thing as Usurpation; but a Highway-Man so long as he has strength to force, has also a Right to require our Obedience" (1706, x). She makes the argument with unmistakable reference to Locke's *Second Treatise*, (sec. 119), where having established that "man is born free," Locke sets out the conditions on which one can be bound to a civil authority. They include *tacit consent*:

> And to this I say, that every Man, that hath any Possession, or Enjoyment, of any part of the Dominions of any Government, doth thereby give his *tacit Consent*, and is as far forth obliged to Obedience to the Laws of that Government, during such Enjoyment, as any one under it; whether this his Possession be of Land, to him and his Heirs for ever, or a Lodging only for a Week; or whether it be barely travelling freely on the Highway; and in Effect, it reaches as far as the very being of any one within the Territories of that Government. (1988, 347–48)

ASTELL, LOCKE, AND THE HIGHWAY MAN: A TEST CASE

Astell cues us for her arguments against Locke by the play she makes of the highway man. It is a test case for her method of refutation, employed against Locke's theological arguments and against his epistemology, particularly evident in her critique in *The Christian Religion* of Locke on "thinking matter" (Springborg n.d.).[7] Locke's strenuous disavowels of the right of conquest endorsed by Hobbes were to no avail with Astell. The preeminent Whig theorist of popular sovereignty, who completed his career by promoting a Dutch conqueror on the English throne and defending royal prerogatives against a Tory Parliament, had destroyed the credentials of his own argument. In *An Impartial Enquiry*, she declares rogue government to be the consequence of Locke's right of self-preservation: "But now, if *Doubts and Suspicions, a Thought, a necessity of Believing, a Prospect and Persuasion, a Meaning of Self-Preservation,* or even Self-Preservation, when Life is really in Danger, can lessen the Guilt of this *Unnatural Rebellion,* and all the *Horrid Facts* it produc'd; what will they not excuse! He who robs upon the High-Way, has his *Prospects,* and *Persuasions,* and *Necessities;* and when he resists the Officers of Justice, he only *means Self-Preservation.*" (1704b, 31)

Astell casts Locke with Milton, "who was a better Poet than Divine or Politician," she says, and whose defence of contract in the *Iconoclastes in Answer to a Book Entitled "Eikon Basilike"* (1649, 237) she rebuts: "And if a Thief meets me on the High-way and goes off with my Purse, therefore he has a Right to it, and GOD Approves the Action!" (1704c, 80). Finally, in *An Impartial Enquiry,* in an ironical parody of Locke on resistance and self-preservation, Astell uses the image again: "He who robs upon the High-Way, has his *Prospects,* and *Persuasions,* and *Necessities;* and when he resists the Officers of Justice, he only *means Self-Preservation*" (1704b, 54).

In an extraordinary series of maneuvers Astell succeeds, by the use of this single metaphor, in conflating separate and incompatible arguments of Locke, collapsing the difference between Locke and Hobbes, and consolidating a case against the Whigs in the popular mind as opportunists if not bandits. This very vulgarization of the Whig position, on which the polarity of party politics to a certain extent depended, had its counterpart in the case that Milton, Locke, and Sidney had mounted against the Loyalists, and on the same ground—as supporters of absolutism, de facto power, and conquest (Pocock 1957, 53–54, 148–50). For while the idiom of the highway appears in Locke's phraseology of the argument for tacit consent, he employs it elsewhere as a synonym for the rogue government of absolute monarchy. The vulgarized Tory position, as Locke paints it, traces a trajectory from the obscurantism of patriarchalism, to the veneration of lineage, to the irrationality of de facto power and the violence of conquest.

For this constellation of ideas the image of the highway man did sterling service. Those sections of the *Two Treatises* (secs. 176, 182, 186, 202, 207), drafted during the Exclusion Crisis of 1679–81, have been recognized as containing specific references to James II.[8] By the 1690s, however, the situation looked considerably different. It was not the prospect of James invading with the power of France behind him but the reality of a Dutch interloper on the English throne with which the parties were now confronted. Suddenly the caricature of defenders of the last Stuart Pretender painted by Sidney, Tyrrell, and Locke bore a strange resemblance to the Whig defence of unconditional allegiance to the House of Orange endorsed by Locke.

In chapter 16 of the Second Treatise, section 176, Locke laid out his case against right of conquest, again with clear reference to James II, claiming that "the *Aggressor,* who puts himself into the state of War with another, and *unjustly invades* another Man's right, *can,* by such an unjust War, *never* come to *have a right over the Conquered*" any more than "Robbers and Pyrates have a Right of Empire over whomsoever they have Force enough to master" (1988, 385). To complete the metaphor, he asks, "Should a Robber break into my House, and with a Dagger at my Throat, make me seal Deeds to convey my Estate to him, would this give him any Title?" And he answers, "Just such a Title by his Sword, has an *unjust Conquerour,* who forces me into Submission."

Without doubt, Locke is making his case in opposition to Hobbes, for whom conquest established legitimate contracts and who first introduced the metaphor of the highway man with the claim that "amongst men, till there were constituted great Commonwealths, it was thought no dishonour to be a Pyrate, or a High-way Theefe" (1991, 67). Hobbes had reflected on the case of the highway man in making his case for the inviolability of contracts: "And even in Common-wealths, if I be forced to redeem my selfe from a Theefe by promising him money, I am bound to pay it, till the Civill Law discharge me" (p. 98).

With unmistakable reference to his own case and the treatment by Charles II of the Shaftesbury circle in the wake of the Rye House Plot, Locke, in section 202 of chapter 13 on tyranny, defends an individual's right to self-preservation against arbitrary arrest as a symptom of arbitrary government, once again compared to highway robbery: "He that hath Authority to seize my Person in the Street, may be opposed as a Thief and a Robber, if he indeavours to break into my House to Execute a Writ, notwithstanding that he know he has such a Warrant, and such a Legal Authority as will impower him to Arrest me abroad" (1988, 401).

To make it clear, indeed, that his case is aimed at defenders of absolutism—and specifically against those who would defend a French-supported invasion by the Stuart Pretender—Locke, in section 177 of the same chapter, boldly states, "They that found Absolute Monarchy upon the Title of the Sword, make their Heroes, who are the Founders of such Monarchies, arrant *Draw-can-Sirs,*" employing a proverbial phrase from the Duke of Buckingham's much-played play, *The Rehearsal* (1663/64), for characters who kill off everybody around them (1988, 387 and 15).

In section 182 of this chapter, Locke further employed the image of the highway man to define the precise limits of rights to self-preservation and the protection of one's property: "For though I may kill a Thief that sets on me in the Highway, yet I may not (which seems less) take away his Money and let him go; this would be Robbery on my side. His force, and the state of War he put himself in, made him forfeit his Life, but gave me no Title to his Goods" (1988, 390).

Possibly here, too, the analogue of royal usurpation is in Locke's mind. In section 186 of chapter 16, the theme is replayed: "He that forces my Horse from me, ought presently to restore him, and I have still a right to retake him" (Locke 1988, 393). But now the highway man is furnished with a pistol, rather than a knife: "Nor does it at all alter the case, to say I *gave my Promise*, no more than it excuses the force, and passes the Right, when I put my Hand in my Pocket, and deliver my Purse my self to a Thief, who demands it with a Pistol at my Breast" (ibid.)

In point of fact, Locke consistently uses the image of the highway man to illustrate the conditions under which the right of resistance may be exercised. They are conditions in which the writ of legal redress does not run. This he clearly spells out in section 207 of the chapter on tyranny, where he expands on his thesis that rule of law is a form of malpractice insurance for the sovereign: it guarantees the loyalty of his subjects. Locke himself notes an anomaly that renders the citizen impotent against a violation of trust that endangers his property but not his life. Expressed in the metaphor of highway robbery, it is an anomaly that is systemic, given Locke's insistence that violence must be met by passive resistance where appeal to the law is available. This principle, designed to protect the sovereign once legally enstated, ensures that the "*Doctrine* of the lawfulness of *resisting* all unlawful exercises of his Power, *will not* upon every slight occasion indanger him, or *imbroil the Government*." This is because "where the injured Party may be relieved, and his damages repaired by Appeal to the Law, there can be no pretence for Force, which is only to be used, where a Man is intercepted from appealing to the Law." So far, so good; but, as Locke freely admits, this leaves the honest citizen hostage to free-riders and outlaws if not to highway men as such. Reading it, one can understand why Astell might have deliberately conflated Hobbes' highway thief, a bandit, with Locke's highway man who was not. Such a disparity in the security offered the sovereign by rule of law, compared with the citizen, held to ridicule the new political order the social contract was said to enstate:

> A Man with a Sword in his Hand demands my Purse in the High-way, when perhaps I have not 12*d*. in my Pocket; This Man I may lawfully kill. To another I deliver 100.*l*. to hold only whilst I alight, which he refuses to restore me, when I am got up again, but draws his Sword to defend the possession of it by force, if I endeavor to retake it. The mischief this Man does me, is a hundred, or possibly a thousand times more, than the other perhaps intended me, (whom I killed before he really did me any) and yet I might lawfully kill the one, and cannot so much as hurt the other lawfully. The Reason whereof is plain; because the one using *force*, which threatned my Life, I could not have *time to appeal* to the Law to secure it: And when it was gone, 'twas too late to appeal. The Law could not restore Life to my dead Carcass: The Loss was irreparable; which to prevent, the Law of Nature gave me a Right to *destroy* him, who had put himself into a state of War with me, and threatned my destruction. But in the other case, my Life not being in danger, I may have the *benefit of appealing* to the Law, and have Reparation for my 100 *l*. that way. (Locke 1988, 403–4)

POLITICAL DEMAGOGUES AND DOMESTIC TYRANTS

Whether by coincidence or not, Mary Astell and Charles Leslie arrived at the same test of veracity for Locke and the Whigs, challenging them to show their sincerity by implementing in the family the criteria for public accountability they applied to the state. In the preface to *Reflections upon Marriage*, in the passage immediately following the remarks about the highway man, Astell drew the analogue between rogue government in the state, scarcely veiled by the fiction of tacit consent, and rogue government in the family, to which no fig leaf is attached, with specific reference to Locke:

> Again, if Absolute Sovereignty be not necessary in a State, how comes it to be so in a Family? or if in a Family why not in a State; since no Reason can be alledg'd for the one that will not hold more strongly for the other? If the Authority of the Husband so far as it extends, is sacred and inalienable, why not of the Prince? The Domestic Sovereign is without Dispute Elected, and the Stipulations and Contract are mutual, is it not then partial in Men to the last degree, to contend for, and practise that Arbitrary Dominion in their Families, which they abhor and exclaim against in the State? (1706, x)

This appears to make direct reference to an argument made by Locke on the right of resistance in section 209 of chapter 13, "Of Tyranny," in the *Second Treatise*, a passage widely agreed to be directed at James II, in which Locke notes of tyrannous acts drawing the analogy to the family:

> This is an *Inconvenience*, I confess, that *attends all Governments* whatsoever, when the Governours have brought it to this pass, to be generally suspected of their People; the most dangerous state which they can possibly put themselves in: wherein they are the less to be pitied, because it is so easie to be avoided; It being as impossible for a Governour, if he really means the good of his People, and the preservation of them and their Laws together, not to make them see and feel it; as it is for the Father of a Family, not to let his Children see he loves, and takes care of them. (1988, 404–5 and n.)

Charles Leslie, in his "Supplement" of 25 March 1703 to *The New Association*, part 2, had given what is believed to be the first systematic commentary on Locke's *Two Treatises* (Thompson 1980). There Leslie made precisely the argument against Locke that Astell makes here, drawing the conclusion that she implies: "These Men whose chief *Topick* is the *Liberty* of the *People*, and against *Arbitrary Power*, are the most *Absolute* of any other in their *Families*, and so [7] Proportionably, as they rise *Higher*. . . . And can any Believe, that a *Tyrant* in a *Family* would not prove the same upon a *Throne*? It has ever prov'd so. And I desire no other *Test* for these Publick *Patrons* for *Liberty*, than to look into their *Conversation* and their *Families*. Then let any Man *Believe* them if he *Can*; and *Trust* them, if he *please*" (1703, 6–7).

The same argument was made by Astell in the Preface to the second edition of *Reflections upon Marriage*:

> For if Arbitrary Power is evil in itself, and an improper Method of Governing Rational and Free Agents it ought not to be Practis'd any where; Nor is it less, but rather more mischievous in Families than in Kingdoms, by how much *100000* Tyrants are worse than one. What tho' a Husband can't [xi] deprive a Wife of Life without being responsible to the Law, he may however do what is much more grievous to a generous Mind, render Life miserable, for which she has no Redress, scarce Pity which is afforded to every other Complainant. It being thought a Wife's Duty to suffer everything without Complaint. If *all Men are born free,* how is it that all Women are born slaves? (1706, x–xi)

Astell's famous concluding aphorism, italicized by her to indicate quotation, seems to make direct reference to Locke's *Two Treatises*. The *First Treatise* had set out to rebut Sir Robert Filmer's (1990) *Patriarcha,* founded, Locke claims on two principles: "that all Government is absolute Monarchy" and "that no Man is Born free" (1988, 142). To the refutation of these principles the entire *First Treatise* is dedicated. The principle that "men are born free" is asserted in the *Second Treatise* (secs. 4, 22, 87, 95, 113, etc.).

Locke was by no means the only thinker of his day to make the case for liberty as a birthright. It was renowned among the radical sects (Thomas 1958, 50) and endorsed by his contemporary Gilbert Burnet (also a possible source for Astell) in his 1689 tract, *An Enquiry into the Measures of Submission to the Supream Authority*. The most liberal of all his writings, published before Locke's *Two Treatises* (1690), this work appears to be the source for Astell's later sarcastic reference to "my Lord of Salisbury" (1704a, 16). Burnet's work begins by specifically excluding women from the natural right to freedom enjoyed by men, allowing voluntary enslavement, also permitted by Hobbes but denied by Locke and Astell:

> It is certain, that the *Law of Nature* has put no difference nor subordination among Men, except it be that of *Children* to *Parents*, or of *Wives* to their *Husbands*; so that with Relation to the Law of Nature, all Men are born free: and this Liberty must still be supposed entire, unless so far as it is limited by Contracts, Provisions and Laws. For a Man can either bind himself to be a Servant, or sell himself to be a Slave, by which he becomes in the power of another, only so far as it was provided by the Contract: since all that Liberty which was not expressly given away, remains still entire: so that the plea for Liberty always proves it self, unless it appears that it is given up or limited by any special agreement. (Burnet 1698, 53)

What allows us to establish with more or less certainty that Astell is, in fact, quoting from Locke and not Burnet? In the continuation of her passage, Astell incorporates further quotation from the *Two Treatises,* once again italicized. As an aside on her rhetorical question "If *all Men are born free,* how is it that all Women are born slaves?," she continues: ". . . as they must be if the being subjected to the *inconstant, uncertain, unknown, arbitrary Will* of Men, be the *perfect Condition of Slavery?* and if the Essence of Freedom consists, as our Masters say it does, in having a *standing Rule to live by?* And why is Slavery so much condemn'd and strove against in one Case, and so highly applauded and held so necessary and so sacred in another?" Here Astell seems to be quoting from two different sections of the *Two Treatises*—book 2, sections 22 and 149:

> . . . A Liberty to follow my own Will in all things, where the Rule prescribes not; and not to be subject to the inconstant, uncertain, unknown, Arbitrary Will of another Man.
>
> For no Man, or Society of Men, having a Power to deliver up their *Preservation*, or consequently the means of it, to the Absolute Will and arbitrary Dominion of another; whenever any one shall go about to bring them into such a Slavish Condition, they will always have a right to preserve what they have not a Power to part with; and to rid themselves of those who invade this Fundamental, Sacred and unalterable Law of *Self-Preservation*, for which they enter'd into Society. (1988, 283–4, 367)

This last is the passage that, in *The Christian Religion* Astell quotes from the *Two Treatises* with acknowledgement (1705, 133). Does this mean, then, that Leslie takes the argument concerning the hypocrisy of advocates of liberty in the public sphere who deny it in the private from Astell? Or is it merely a coincidence? If sheer density of argumentation were considered, the systematic rebuttal of Locke that Astell undertook throughout her corpus would constitute not only the first but perhaps also the most sustained contemporary critique of Locke's *Two Treatises*, crossing back and forth from the *Reasonableness of Christianity* to the *Essay Concerning Human Understanding*, the *Letters on Toleration*, and Locke's letters to Stillingfleet.

ASTELL'S CRITIQUE OF LOCKE: THE SIGNIFICANCE

The function of Locke's argument in the very fluid situation leading up to 1688 was not lost on Mary Astell: it was to disintangle political obligation from dynastic allegiance and break the knot of conflicting oaths undertaken to the Commonwealth, to the Stuarts, and, subsequently, to William III. The image of the highway man was perhaps unfortunate, a sign of rogue government of which Astell made the most. But dynastic uncertainty and fears of a relapse into civil war and anarchy were very real from the 1660s through to the establishment of the Hanoverians. Locke and the Whigs had their own solution to the dynastic problem, which combined pragmatism and conciliation of the religious parties, a cost too high for the Tories. Locke's priority in the 1680s, the subject of recent scholarly scrutiny, had been to cut a swathe through the conflicting oaths that tied subjects to the Stuart dynasty, by endorsing the right of citizens to give or withhold their consent to dynastic change. He rested his case on the eloquently expressed argument against slavery: "For a Man, not having the Power of his own Life, *cannot*, by Compact, or his own Consent, *enslave himself* to any one, nor put himself under the Absolute, Arbitrary Power of another, to take away his Life, when he pleases. No body can give more Power than he has himself; and he that cannot take away his own Life cannot give another power over it." (1988, 284)

Astell uses Locke's argument against him to make the case for a distinction between authority and title, on assumptions that are otherwise directly contrary to Locke on authorization, restating Scholastic arguments for a distinction between authorization and designation in the capacity of the people to empower a government. These were the very arguments put to Locke by William Sherlock, on whose views Locke commented in a draft essay that has survived (reproduced in Goldie 1978, app.). Although Astell could not have known about this document, it gives an indication of how close to the mark she came in her critique of Locke. People may choose the person of the governor, but they cannot empower him, Astell argued, because "none can give what they have not: The People have no Authority over their own Lives, consequently they can't invest such an Authority in their Governours" (1704, 18). The argument with which Astell then proceeds seems to be explicitly aimed at Locke: "And tho' we shou'd grant that People, when they first enter into Society, may frame their Laws as they think fit; yet these Laws being once Establish'd, they can't Legally and Honestly be chang'd, but by that Authority in which the Founders of the Society thought fit to place the Legislature. Otherwise we have been miserably impos'd upon by all those Arguments that were urg'd against a Dispensing Power" (1704b, 18).

Locke, in fact, came to agree, insisting by 1690 on the oath of allegiance to William as a condition of citizenship, threatening to ostracize the nonjurors, or those who refused to take it. Astell, on the other side, shows Locke and the radical Whigs to have been opportunists, who did not stoop to exaggerate the threats posed to the regime by those who refused to swear allegiance—a systemic irony, given the official freedom to consent that Locke and his like endorsed:

> The *Innuendo* (if one may be so bold as to unveil it) is, that Men in Office and Power, are of the same Principles of the Present *Non-Jurors*, if there are any such, and minister to some Design, which we are pleas'd to fansie for them; tho' the poor Men have neither Power to Accomplish, nor if we will believe them, the Will to contrive any. But that *Innuendo* is a Calumny as ridiculous and unreasonable, as it is groundless and without any manner of Proof, scarce any Colour, but that which the good Fore-heads of the Inventors give it. Because whatever the Pretended Reasons for Revolutions may be, the true Reason is always the Change of hands; that Party which was neglected endeavouring to get into the Saddle. (1704c, 105)

In *Moderation Truly Stated*, Astell seems to trace the contours of Locke's career—at first supporter of the Anglican establishment, then, as he fell under Shaftesbury's influence, a proponent of resistance, and finally a propagandist for William III. Who but Locke made such "a bustle about Liberty and Property," after all?

> And for what End and Purpose is all this ado? for what do they Rent the Church of Christ, and tear the Bowels of their Country by intestine Broils? for what but to advance themselves, to gratify their Passions and their Vices! The People never get by Divisions and Revolutions, they lose their Peace and Quiet, their Money is exhausted in Taxes, and their Blood in War, only to raise a few *New Men*, and that the cunning Folks who manage all, may make their Markets. So that all their goodly Pretences of Redress of Grievances, and their bustle about Liberty and Property, have no other meaning, no other Conclusion, but their Own Advancement. (1704c, 93)

Locke is the typical Whig, at all times putting party and preferment over principle. Astell would seem to refer to Locke's counsels to William III, when she continues:

> The truth is, as he can be no Friend to a Prince, who fills his Head with projects of Arbitrary Power, and carries him beyond those bounds the Laws have set him; for whether or no the People *ought* to Resist in such a Case, yet to be sure they *will*: so on the other side, he is no true Patriot who would invest the People with a Power they know not how to manage, teaching them to Mate their Superiors, and by withdrawing their necessary Subjection from their Lawful Prince, certainly tho' silly, and under the specious name of Liberty, enslaves them to a few seditious Demagogues, and Popular Harranguers. (ibid.)

In *The Christian Religion*, published a year later and perhaps already under way, there are frequent and unmistakable references to Locke as a party man whose politics drive his theology. In this work, in which Astell treats together *The Essay on Human Understanding*, Locke's Letters to Stillingfleet on the *Essay*, *The Reasonableness of Christianity* and the *Vindication of the Reasonableness of Christianity*, Masham's *Discourse Concerning the Love of God*, the anonymous *Ladies Religion*, and Locke's *Two Treatises*, as if all were by the same hand,[9] Astell characterizes Locke as a Socinian, an Epicurean, a party man, and a defender of liberty, property, choice, and Dissent. Her characterizations usually begin with a satirical denial and then immediately proceed to assume the proposition that has been denied. So in the very passage in which she acknowledges treating these works indiscriminately as Locke's she disingenuously disavows slander:

> I do not accuse that Author of *any tang of Prepossession or Phansy;* any *footsteps of Pride or Vanity; any touch of Ostentation or Ambition, any thing tending to his own bye-interest, or that of a Party;* as if these had *tempted* him *to mix any conceits* with the Christian Religion (p. 185.), or to give any wrong turn to the Faith of the Gospel. And I hope I have not treated him otherwise than becomes a Christian, whose Charity is the substance of what Good-breeding is only the shadow and counterfeit. (1705, 82)

Astell betrays her great cleverness in later levying the charge against her nameless opponent, clearly Locke, repesentative of the country gentleman, of "unsufferable Insolence . . . from which even Good-breeding, the only Religion that some People have, shou'd methinks restrain them, if Reason and Religion do not weigh with them," thus canceling her disavowal (1705, 154). She openly accuses the author of the *Discourse* of tailoring his (in fact, it was Masham's) theology to his politics:

> everything is not True which we find in the *Discourses* of our Modern Authors, who not only refine upon Philosophy, by which they do Service to the World; and upon Politicks, by which they mean to Serve their Party; but even upon Christianity it self, pretending to give us a more *Reasonable* Account of it, by which they mean somewhat more agreeable to their Genius and own Conveniency, for their Systems, so far as I can find, do no manner of *Service to decaying Piety, and mistaken and slander'd Christianity.*[10] (1705, 135).

Allowing for her own partisan position, the picture that emerges of Locke and the typical Whig in Astell's writings is a window onto an eighteenth century Locke that in the absence of much contemporary reference, we have been lacking too long. It is not a pretty picture and is undoubtedly only one among many partisan portraits, but it is certainly a necessary antidote to the sanitized Locke, father of liberalism, of more recent acquaintance. The answer to the question Did Astell know the authorship of the *Two Treatises*? is probably *yes*. Locke's authorship was a widely circulated secret in the 1690s. Charles Leslie—to whose *New Association* (1702–3), Astell made reference in *A Fair Way with the Dissenters* (1704a, 15)—had, in the Supplement to *The New Association, Part II* (dated 25 March 1703), undertaken one of the first critiques of the *Two Treatises* (Thompson 1976, 187, 189). Earlier, Leslie had observed Locke's hand in the work of Molyneux, *The Case of Ireland's Being Bound by Acts of Parliament in England, Stated* (1698), mounting against him the Lockean argument that Irish parliaments had "consented" to English rule by swearing allegiance to William and Mary in 1692 and 1695 (cited in Thompson 1976, 187, 189). Astell's own use of Lockean language and metaphor of the *Two Treatises* as early as 1700 and her reuse of the same material with acknowledgement in *The Christian Religion* of 1705 along with other known works of Locke suggest that she considered this work to be by her adversary as well.

Questions have been raised about the reasons for Astell ceasing to write after 1709—the completion date of *Bart'lemy Fair*, her critique of the *Letter Concerning Enthusiasm* (1708) by Anthony Ashley Cooper, the third earl of Shaftesbury (Perry 1986, 172–73). Addressed to Lord Somers (whose name is not given) and occasioned by the "French prophets," it pretends to be the work of Wotton. Anthony Ashley Cooper, grandson of Locke's patron, the first earl of Shaftesbury, had been Locke's pupil. It is my surmise that when Locke and his projects died, Astell's urge to write died with them. The Tory cause became ascendant in 1710 and no longer required such embattled defences. Only after 1714 was it to succumb to Locke and the Whigs, ultimate winners of the struggle for constitutional and historical hegemony.

Astell's *Reflections upon Marriage* (1700) is one of the earliest critiques of the cornerstones of early modern political theory—patriarchalism, contractarianism, and the political philosophers who promoted them: Filmer, Hobbes, and Locke. Marriage provided a juridical formula for the union of two wills, such that parties entering as free and equal individuals emerged in a relationship of domination/subordination. But problems lay in replicating these features in a social contract that was compatible with natural right and freedom as the contractarians defined it (Hinton 1967, 1968; Pateman 1988, 1989; Shanley 1979). Reason and power belonged to fundamentally different orders: if reason could not sanction tyrants, it could not disenfranchise them either. This Astell, addressing Locke, was able to demonstrate. The enduring significance of her critique lies, as Carole Pateman (1988) has correctly diagnosed, in a prospective critique of contractarianism as the foundation of liberalism. It is not for the "trade in bodies"—conjugal right dressed up as equality—that Astell condemns the marriage contract/social contract homology. Rather, it is because contractarianism, as defining free and equal individuals, is predicated on models of contract in which these conditions do not apply. They do not apply in the social contract, which produces a political order of domination and subordination out of the initial freedom of the war of all against all; and they do not apply in marriage either, in which hierachical social relations sanctioned by divine authority are merely occasioned by a "contract" between the families designating partners to this estate. The power to authorize marriage, like the power to authorize a sovereign, is, in her view, a power assigned only to God. The power of his representatives is limited to designating incumbents to these respective estates.

Out of the many scores of works devoted to the subject of marriage, Hobbes' *Leviathan* had run a radical line in rendering family relations contractual. Hobbes did so to deny categorial differences in social order theorized by Aristotle in the distinction between paternal, despotic, and political power, institutionalized by feudal Christendom in general and the Catholic Church in particular. In this respect he was at least more thoroughgoing than Locke in meeting the requirements that the marriage contract/social contract homology required. Astell made Locke's failure of nerve the butt of ridicule. But she did so not to transform social relations but, rather, to critique eighteenth-century morals and mores that made a mockery of the sacrament of marriage. In richly written rhetorical passages she mounts a vehement protest against the estate of marriage in her day—language marked by images of freedom and slavery that her ontological commitments did not allow her to translate into political theory. To this cry of protest against a cruel world corresponds her theology, marked by Cartesian rationalism and Platonist enthusiasm in which freedom of the spirit is given religious, rather than political, expression.

Notes

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1. Astell 1704b, 40; idem 1705, 262–63, etc., citing the *Two Treatises*, "Mr Lock's *Third Letter to the Bishop of Worcester*, p. 409," and his *Essay* throughout. Until the death of William III, Locke's employer, Astell showed customary reluctance to name Locke; and even in 1704 she frequently attacked his arguments through surrogates.

2. See Ashcraft 1986, 1992; Farr and Roberts 1985; Friedman 1988; Goldie 1978, 1980a, 1980b, 1992; McNally 1989; Nelson 1978; Schwoerer 1990; Thompson 1976, 1987, 1980; Wootton 1992.

3. Note that Thompson (1980) revises his 1976 position in light of Nelson's 1978 critique.

4. For instance, Astell, in *An Impartial Inquiry* mocks the language of the country Whig, exemplified by John Tutchin (1661?–1707), who combined reverence for the Ancient Constitution, Parliament, and "Native Right" with xenophobia, declaring of the constitution, "She's as well beloved now by all true *Englishmen*, as she was by our Forefathers a Thousand Years ago" (1704b, 8). His views were set out in the *Observator* from 29 September to 7 November 1703, focusing on resistance and targeted at Charles Leslie (see Phillipson 1993, 217). They were bound for this reason to have come to the attention of Astell, who gives the impression that Kennett held the same views. Tutchin admired "those two great men, Mr. Sidney and Mr. Lock," defenders of ancient liberty, "the one against Sir Robert Filmer, and the other against a whole Company of Slaves." But the only occasion on which he names Locke (1706; see Phillipson 1993, 218), is too late for Astell's pamphlet.

5. See Locke's minute to Edward Clarke, Bodleian MS Locke e18; Farr and Roberts 1985, 397; and Goldie 1991.

6. Cf. Astell 1706, xi. I thank John Pocock for his query about the status of the word "slave" in Astell. My answer is that it must be ironic. Astell denied Locke's claim to property in one's person; a claim that applied in his day only to men. In Locke this right is crucially important in the negative: as the incapacity of individuals [men] to incur hereditable impediments to their freedom or voluntarily to enter into slavery. To the extent that a positive right was entailed, it was part of a package tailored by Locke to undermine hereditable monarchy. Since Astell denied property in one's person, vouched for in Locke by the right to real property (a right that women were denied), she could not technically argue the slavery of women compared with the freedom of men. Nor does she seriously attempt to do so. This marks a difference between her work and that of Judith Drake, whose *Essay in Defence of the Female Sex* seriously advances a comparison between early modern married women and "our Negroes in our Western Plantations" (1696, 22). Drake's tract is often attributed to Astell and was listed in Locke's library along with *Reflections upon Marriage* as her work (see Harrison and Laslett 1971, items 1104–5, 1914).

7. See the thoughtful essays on Locke and "thinking matter"—Squadrito 1987 and Scaltsas 1990.

8. *The Second Treatise*, chap. 16, "Of Conquest" (1988, 384). See Laslett's note on section 175 as belonging to "the early stages of composition" and "probably written in 1681 or 1682," although arguably inserted in 1689.

9. Astell's form of words suggests that she may have known, or suspected, Masham's authorship. So in section 87 of *The Christian Religion*, Astell expressed her doubts about the author of the *Discourse*: "Whether he be the same Person who writ *A Discourse concerning the Love of GOD*, or who is the Author, it is not my business to enquire, since he has not thought fit to discover himself." (1705, 82). The opening paragraph of *The Christian Religion* refers enigmatically to the Press having "help'd us to the Religion of a *Physician*, a *Layman*, a *Gentleman*, and a *Lady*, yet in my poor opinion, they have all of them but one Religion if they are Christians" (p. 2). The query "if they are Christians" may refer to the charge by John Edwards, in *Socinianism Unmasked* (1696), that Locke was first a Socinian and second a Muslim (Champion 1992, 111)—a charge that Astell repeats, accusing defenders of Alcoran of "a Religion accommodated even to the lees and dregs of Sense, [that] obtains among too many sensual men" (p. 22).

10. Other references to the defence of liberty, property, interest, reputation, sensibility, and status are to be found at sections 128, 180, 182, 224, 239, 341, 386 (1705, 120, 179, 181, 217, 232, 335, 383). At section 386 Astell declares: "We think our selves Saints if we are Zealots for a Party; and that we do GOD great Service when we labour to make Proselytes, not to Religion but to our Faction" (p. 383). The ascription of this view to Locke seems likely from the drift of her following remarks that ideologues are prone to idol smashing and sacrilege, all in the name of reasonableness, to dramatize their cause (ibid.). Elsewhere Astell suggests that politicians who dared to "dispossess Lawful Sovereigns, who destroy GOD's Heritage, and root up the Order and Government of His Church *shall Rot*"—in Hell, presumably, as if she subscribed to mortalism, which she does not (p. 143).

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WHAT TO DO (AND NOT TO DO) WITH TIME-SERIES CROSS-SECTION DATA

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We examine some issues in the estimation of time-series cross-section models, calling into question the conclusions of many published studies, particularly in the field of comparative political economy. We show that the generalized least squares approach of Parks produces standard errors that lead to extreme overconfidence, often underestimating variability by 50% or more. We also provide an alternative estimator of the standard errors that is correct when the error structures show complications found in this type of model. Monte Carlo analysis shows that these "panel-corrected standard errors" perform well. The utility of our approach is demonstrated via a reanalysis of one "social democratic corporatist" model.

We shall show that a commonly used technique for the analysis of time-series cross-section (TSCS) data produces incorrect results. Our result either invalidates or calls into question the findings of at least five articles published in the *American Political Science Review*, as well as a like number in other leading journals in political science and sociology. Table 1 provides an incomplete list of relevant articles whose conclusions are based on the use of this problematic technique. All of these articles use an application of the generalized least squares (GLS) method first described by Parks (1967), a method designed to deal with some common problems that occur in TSCS data. We show that the Parks method produces dramatically inaccurate standard errors when used for the type of data commonly analyzed by students of comparative politics. We then offer a new method that is both easier to implement and produces accurate standard errors.

Time-series cross-section data are characterized by having repeated observations on fixed units, such as states or nations. The number of units analyzed would typically range from about 10 to 100, with each unit observed over a relatively long time period (often 20 to 50 years). Both the temporal and spatial properties of TSCS data make the use of ordinary least squares (OLS) problematic. In particular, models for TSCS data often allow for temporally and spatially correlated errors, as well as for heteroscedasticity. Parks proposed a method for dealing with these problems based on GLS.[1] The use of this method can lead to dramatic underestimates of parameter variability in common research situations.

Why the severe problems with the Parks method? Is it not just an application of well-known GLS? While GLS has optimal properties for TSCS data, it assumes that we have knowledge about the error process that, in practice, we never have. Thus analysts use not GLS, but feasible generalized least squares (FGLS). It is "feasible" because it uses an estimate of the error process, avoiding the GLS assumption that the error process is known. The FGLS formula for standard errors, however, assumes that the error process is known, not estimated. In many applications this is not a problem because the error process has few enough parameters that they can be well estimated. Such is not the case for TSCS models, where the error process has a large number of parameters. This oversight causes estimates of the standard errors of the estimated coefficients to understate their true variability. We provide a measure of how much the Parks standard errors understate true sampling variability, that is, how much the Parks method falsely inflates confidence in the findings of TSCS studies.

Unfortunately, it is not possible to provide analytic formulae for the degree of overconfidence introduced by the Parks method. Instead, we provide evidence from Monte Carlo experiments using simulated data to assess the performance of the various estimators. This evidence clearly shows the overconfidence induced by the Parks method. The Parks estimator may understate variability by between 50% and 300% *in practical research situations*. It is this extreme overconfidence that leads us either to overturn or to cast doubt on the findings of many analyses based on the Parks method.

Having demonstrated the problems of the Parks method, we instead advocate a simpler method for estimating TSCS models. It is well known that even though OLS estimates of TSCS model parameters may not be optimal, they often perform well in practical research situations. It is also well known that the OLS estimates of standard errors may be highly inaccurate in such situations. We therefore propose to retain OLS parameter estimates but replace the OLS standard errors with *panel-corrected standard errors*. Monte Carlo analysis shows that these new estimates of sampling variability are very accurate, even in the presence of complicated panel error structures.

We shall detail the problems of the Parks method, laying out the structure of TSCS models and showing why OLS is problematic. In order to understand Parks' solution and why it is problematic, it is neces-

TABLE 1

Articles Using the Parks Method

CITATION	N[a]	T[b]	MODEL
Number of Units Is Less Than Number of Time Points			
Hicks & Swank 1992	18	23	Political determinants of welfare spending in advanced industrial societies
Hicks 1994b[c]	17	18	Political and union determinants of economic growth in advanced industrial societies
Pampel 1993[c]	18	36	Political and social determinants of fertility rates in advanced industrial society
Schneider & Ingraham 1984	7, 18	59	Political determinants of social program expansion in advanced industrial societies
Su, Kamlet & Mowery 1993[d]	5, 7, 10,11	26	Political determinants of U.S. budget by category
Swank 1992	16	20	Political determinants of tax policy in advanced industrial societies
Number of Units Exceeds Number of Time Points[e]			
Alvarez, Garrett & Lange 1991	16	15	Political and union determinants of economic growth in advanced industrial societies
Friedland & Sanders 1985[c]	12	6	Political determinants of economic growth in advanced industrial societies
Giles and Hertz 1994[c,f]	64	15	Party registration and race in Louisiana parishes
Lin 1994[c]	20, 42, 62	26	Influence of government spending on cross-national economic growth
Pampel & Williamson 1988[c]	18	7	Political determinants of welfare spending in advanced industrial societies
Scholz, Twombly & Headrick 1991	30	10	Political determinants of enforcement of regulatory standards in New York counties
Scholz & Wei 1986	50	8	Regulatory enforcement in the states
Wood 1992	50	9	Policy implementation in the states

[a] Number of cross-sectional units.
[b] Number of time periods.
[c] Used Parks in addition to other methods; estimates using other methods not discussed in this article.
[d] Estimated with seemingly unrelated regressions; same as Parks correction for contemporaneously correlated errors.
[e] Parks method cannot produce results.
[f] Article indicates use of Park. Private communication with the authors indicates that Park was not in fact used.

sary first to consider some properties of FGLS. The Parks method is then laid out. This method provides an estimation strategy that takes into account both cross-sectional and temporal complications of the data. These two components of the method are presented and assessed separately. These assessments show that the Parks standard errors are likely to lead to extreme overconfidence for typical TSCS data, with the correction for cross-sectional complications being much more problematic than the correction for temporal complications.

We shall then present our proposed method for estimating TSCS models. We argue that this method, which combines ordinary least squares parameter estimates with panel-corrected standard errors, *should* perform well. As with the Parks method, a full assessment of this method for the types of data encountered in research situations is only possible via Monte Carlo analysis.

We then present this Monte Carlo evidence. Our results demonstrate the extreme overconfidence induced by the Parks standard errors. Our simulations show that while the Parks correction for cross-sectional complication causes much of the problem, the correction for temporal complications is also problematic. In addition, the Monte Carlo evidence shows that panel-corrected standard errors perform extremely well, even in the presence of complicated panel error structures. The Monte Carlo evidence also shows that OLS parameter estimates are themselves, at worst, not much inferior to the Parks parameter estimates. Thus the costs of the inaccurate Parks standard errors are in no sense paid for by the superiority of the Parks estimator of the model parameters.

Finally, we use our proposed method to reanalyze Hicks and Swank's (1992) results obtained with the Parks method. We show that the strengths of many of their findings about the political causes of government spending in advanced industrial societies are an artifact of their use of the Parks method. We also briefly reconsider the findings of other studies that used that method. The conclusion presents a unified method for analyzing TSCS data.[2]

THE PARKS METHOD AND ITS FLAWS

Our analysis is limited to what Stimson (1985) called temporally dominated TSCS models, where a limited number of units are observed for a relatively long period of time.[3] The critical assumption of TSCS models is that of "pooling"; that is, all units are characterized by the same regression equation at all points in time. Given this assumption, we can write the generic TSCS model as

$$y_{i,t} = \mathbf{x}_{i,t}\beta + \epsilon_{i,t};\ i = 1, \ldots, N;\ t = 1, \ldots, T \qquad (1)$$

where $\mathbf{x}_{i,t}$ is a vector of one or more (k) exogenous variables and observations are indexed by both unit (i) and time (t).[4] We shall denote the matrix of independent variables for all observations as $\mathbf{X}$ and the vector of observations on the dependent variable as $\mathbf{Y}$. We assume that the data are stacked by unit.[5] We denote the $NT \times NT$ covariance matrix of the errors with typical element $E(\epsilon_{i,t}\epsilon_{j,s})$ by $\mathbf{\Omega}$. TSCS models can be difficult to estimate because the error process of such models may be more complicated than is typical of either time-series or cross-sectional models. Different assumptions about this error process lead to different preferred methods of estimation.

Ordinary Least Squares Is Problematic for Time-Series Cross-Section Data

Ordinary least squares is optimal (best linear unbiased) for TSCS models if the errors are assumed to be generated in an uncomplicated ("spherical") manner. In particular, for OLS to be optimal it is necessary to assume that all the error processes have the same variance (homoscedasticity) and that all of the error processes are independent of each other. The latter assumption can be broken down into the assumption that errors for a particular unit at one time are unrelated to errors for that unit at all other times (no serial correlation) and that errors for one unit are unrelated to the errors for every other unit (no spatial correlation). Under these assumptions TSCS models should be estimated by OLS and OLS standard errors are correct. Most analysts, however, are not willing to accept the assumption of spherical errors for TSCS models.

Ordinary least squares is not optimal in the presence of nonspherical errors, in the sense that there will be other estimators that make more efficient use of the data. More seriously, if the errors are not spherical, there is no guarantee that the OLS standard errors will be correct. We use the term *correct standard errors* to indicate that we have accurate estimates of the variability of parameter estimates. Correct standard errors allow for the correct computation of confidence intervals and statistical tests. Incorrect standard errors will lead us to be either too confident or insufficiently confident about whether our findings might merely be statistical artifacts.

It is, of course, always possible that the errors of any regression model may be nonspherical. The problem is, however, much more acute for TSCS models. In particular, we might expect TSCS errors to be contemporaneously correlated in that large errors for unit i at time t will often be associated with large errors for unit j at time t. This is likely in the cross-national context, where the economies of, say, the Netherlands and Belgium are linked. It is also a likely problem in other TSCS contexts, such as the study of disaggregated budgets, where large errors in one budget category may be associated with large errors in other categories in the same year. These contemporaneous correlations may differ by unit. For example, the errors in the Scandinavian economies may be linked together but remain independent of errors in the southern European countries.

We might also expect the errors in TSCS models to show "panel heteroscedasticity," where the variances of the error process differ from unit to unit. The errors of a cross-national panel study, for example, may show panel heteroscedasticity because the scale of the dependent variable, such as the level of government spending, may differ between countries. The assumption of panel heteroscedasticity is more stringent than just cross-sectional heteroscedasticity because we continue to assume that the error variances within each unit do not differ over time; this assumed structure allows for certain estimation strategies not available in the nonpanel case.

Finally, it is possible that the errors may show temporal dependence. The most typical assumption is that the errors show first-order serial correlation. Some analysts assume that the degree of serial correlation differs from unit to unit, while others assume it is constant across units.

Time-series cross-section analysts do put some structure on the assumed error process. In particular, they assume that for any given unit, the error variance is constant, so that the only source of heteroscedasticity is differing error variances across units. Analysts also assume that all spatial correlation both is contemporary and does not vary with time. The temporal dependence exhibited by the errors is also assumed to be time-invariant and may also be invariant across units. All of these assumptions allow analysts to attempt to improve on OLS for TSCS data. Since these assumptions are all based on the panel nature of the data, we call them the *panel error assumptions*.[6]

Feasible Generalized Least Squares

Equation 1 can be estimated by generalized least squares regardless of any complexities of the error process, so long as the covariance matrix of those errors, $\mathbf{\Omega}$, is known (up to a scale factor). Given that assumption, GLS is fully efficient and yields consistent estimates of the standard errors (see, e.g., Kmenta 1986, 609–16). Generalized least squares works by transforming equation 1 with a general error covariance matrix to another linear equation where the error covariance matrix is suitable for OLS estimation (spherical). The GLS estimates of β are given by

$$(\mathbf{X}'\mathbf{\Omega}^{-1}\mathbf{X})^{-1}\mathbf{X}'\mathbf{\Omega}^{-1}\mathbf{Y} \qquad (2)$$

with estimated covariance matrix

$$(\mathbf{X}'\mathbf{\Omega}^{-1}\mathbf{X})^{-1}. \tag{3}$$

The problem is that the covariance matrix of the errors, $\mathbf{\Omega}$, is never known in practice (even up to a scale factor). Thus an estimate of $\mathbf{\Omega}$, $\hat{\mathbf{\Omega}}$, is used in expressions 2 and 3. This procedure, FGLS, provides consistent estimates of β if $\hat{\mathbf{\Omega}}$ is estimated by residuals computed from consistent estimates of β. Ordinary least squares provides such consistent estimates. We denote the FGLS estimates of β by $\tilde{\beta}$. The application of FGLS to TSCS models with panel errors was first described by Parks (1967).

Feasible generalized least squares performs well in large samples. In the limit, it is equivalent to full maximum likelihood, and so has all the optimal asymptotic properties of maximum likelihood (Cramer 1986, 79–128). We know little about the finite sample properties of FGLS other than that it yields unbiased estimators under very general conditions that are usually met in practice (Kakwani 1967). The better the estimate of $\mathbf{\Omega}$, of course, the better the FGLS estimator; obviously, estimation of $\mathbf{\Omega}$ will improve as NT increases in relationship to the number of parameters in $\mathbf{\Omega}$ that must be estimated.

It is difficult to assess the performance of FGLS in finite samples. There are by now many Monte Carlo studies showing that FGLS may be less efficient than its OLS counterpart, especially in very small samples. Our interest is in how well FGLS estimates of variability (expression 3) perform in finite samples. It is known that FGLS standard errors underestimate true variability, at least for normal errors (Freedman and Peters 1984). There are, unfortunately, no analytic results that indicate whether this underestimate is of importance to applied researchers, nor, in particular, are there any analytic results about the performance of the Parks estimates of variability for TSCS models. While we will assess this variability with Monte Carlo experiments, we can get some hint about this variability by closer examination of the Park method.

The Parks Method

The Parks method is FGLS for TSCS models where the errors show panel heteroscedasticity, contemporaneous correlation, and unit specific serial correlation. The correction for contemporaneous correlation of the errors automatically corrects for any panel heteroscedasticity, so we need only consider the corrections for contemporaneous and serial correlation of the errors here.[7]

The Parks method consists of two sequential FGLS transformations, first eliminating serial correlation of the errors then eliminating contemporaneous correlation of the errors. This is done by initially estimating equation 1 by OLS. The residuals from this estimation are used to estimate the unit-specific serial correlation of the errors, which are then used to transform the model into one with serially independent errors.[8] Residuals from this estimation are then used to estimate the contemporaneous correlation of the errors, and the data is once again transformed to allow for OLS estimation with now spherical errors.[9] We can therefore consider the consequences of the two corrections separately.

Correcting for Contemporaneous Correlation of the Errors. We first consider the Parks correction for contemporaneously correlated errors. The TSCS model with contemporaneously correlated errors is then exactly expression 1 with the variance covariance matrix of the errors having zeros for all noncontemporaneous observations and free parameters allowing for contemporaneous pairwise correlation of the errors and panel heteroscedasticity. We can write this compactly as $\mathbf{\Omega} = \Sigma \otimes \mathbf{I}_T$, where Σ is the $N \times N$ matrix of contemporaneous covariances, with typical element $E_t(\epsilon_{i,t}\epsilon_{j,t})$.[10] While these parameters differ among pairs of units, they do not vary by time. Feasible generalized least squares, therefore, requires estimating all the pairwise contemporaneous covariances. The matrix of all these estimates is denoted $\hat{\Sigma}$. There are $N \times (N + 1)/2$ contemporaneous covariances; each of these is estimated using NT observations.

The Parks correction for contemporaneously correlated errors cannot be used unless T is at least as big as N (Beck et al. 1993).[11] But even when T is greater than N, so that FGLS can be used, estimation of standard errors is problematic unless T is considerably larger than N. Each element of the matrix of contemporaneous covariances of the errors is estimated using, on average, $2T/N$ observations. Many cross-national panel studies have ratios of T to N very close to 1, so covariances are being estimated with only slightly more than two observations per estimate! Studies on the political economy of advanced industrial nations seldom have T to N ratios that exceed 3; thus the elements of the covariance matrix of the errors are estimated with, on average, six observations. Theory does not tell us how inaccurate the Parks method is in these cases, but we should be prepared to see highly overconfident Parks standard errors in the typical cross-national panel case. We shall provide Monte Carlo evidence of this.

Correcting for Serially Correlated Errors. The Parks correction for serially correlated errors assumes the errors follow a unit-specific first-order autoregressive (AR1) process

$$\epsilon_{i,t} = \rho_i \epsilon_{i,t-1} + \nu_{i,t} \tag{4}$$

where the $\nu_{i,t}$s are (mean zero) variables independently distributed across time. Some analysts impose the additional assumption that the ρ_i are homogeneous across units, that is, $\rho_i = \rho$. Ordinary least squares residuals are used to estimate either the common ρ or the ρ_i; this estimate is then used to transform the data, using the well-known Prais-Winsten transformation (see, e.g., Kmenta 1986, 304).

The FGLS correction for a single ρ requires estimating one extra, unaccounted-for parameter. This is unlikely to cause FGLS standard errors to estimate variability inaccurately in the typical cross-national panel situation. The FGLS correction for unit-specific serially correlated errors, used by Parks, is likely to cause more serious underestimates of variability. The essence of the problem is that each ρ_i is estimated using an autoregression based on only T observations. It is well known that such estimates are biased downward (Hurwicz 1950). As a consequence, the Parks estimates, which correct based on these inaccurate autoregressions, may be inferior to OLS estimates. The underestimates of the ρ_i, when combined with trending data, can cause the Parks estimates of standard errors to misestimate variability substantially.

The assumption of unit-specific serial correlations also seems odd at a theoretical level. Time-series cross-section analysts assume that the "interesting" parameters of the model, β, do not vary across units; this assumption of pooling is at the heart of TSCS analysis. Why should we expect the "nuisance" ρ to not show similar pooling? ρ can be interpreted as how long it takes for prior shocks to be removed from the system. Why should this "memory" be the only model parameter that varies from unit to unit?

The choice whether to correct for serially correlated errors assuming either heterogeneous or homogeneous ρ depends on the small sample properties of the two types of estimators. While we would expect the unit-specific serial correlation correction to lead to more inaccurate estimates of variability, it is also possible that allowing for variation among the ρ_i might improve overall estimation. We can only assess the small sample performance of the two corrections for serial correlation through Monte Carlo experimentation. Before looking at the results of those experiments, we first consider a new method for estimating the variability of OLS estimators. We can then compare the performance of the Parks estimator with our new method.

ORDINARY LEAST SQUARES WITH PANEL-CORRECTED STANDARD ERRORS

If the errors in equation 1 meet one or more of the panel error assumptions, then OLS estimates of β will be consistent but inefficient; the degree of inefficiency depends on the data and the exact form of the error process. The OLS standard errors will also be inaccurate,[12] but they can be corrected so that they provide accurate estimates of the variability of the OLS estimates of β. This correction takes into account the contemporaneous correlation of the errors (and perforce heteroscedasticity). Any serial correlation of the errors must be eliminated before the panel-corrected standard errors are calculated. The correction for contemporaneous correlation of the errors is only possible because we have repeated information on the contemporaneous correlation of the errors; our proposed method does not work outside the TSCS context.

The correct formula for the sampling variability of the OLS estimates is given by the square roots of the diagonal terms of

$$\mathrm{Cov}(\hat{\beta}) = (\mathbf{X}'\mathbf{X})^{-1}\{\mathbf{X}'\mathbf{\Omega}\mathbf{X}\}(\mathbf{X}'\mathbf{X})^{-1}. \qquad \textbf{(5)}$$

If the errors obey the spherical assumption, this simplifies to the usual OLS formula, where the OLS standard errors are the square roots of the diagonal terms of $\widehat{\sigma^2}\ (\mathbf{X}'\mathbf{X})^{-1}$, where $\widehat{\sigma^2}$ is the usual OLS estimator of the common error variance, σ^2. If the errors obey the panel structure, then this formula provides incorrect standard errors. Expression 5, however, can still be used, in combination with that panel structure of the errors, to provide accurate, *panel-corrected standard errors* (PCSEs).[13]

For panel models with contemporaneously correlated and panel heteroscedastic errors, $\mathbf{\Omega}$ is an $NT \times NT$ block diagonal matrix with an $N \times N$ matrix of contemporaneous covariances, $\mathbf{\Sigma}$, along the diagonal. To estimate equation 5, we need an estimate of $\mathbf{\Sigma}$. Since the OLS estimates of expression 1 are consistent, we can use the OLS residuals from that estimation to provide a consistent estimate of $\mathbf{\Sigma}$. Let $e_{i,t}$ be the OLS residual for unit i at time t. We can estimate a typical element of $\mathbf{\Sigma}$ by

$$\hat{\Sigma}_{i,j} = \frac{\sum_{t=1}^{T} e_{i,t}e_{j,t}}{T},$$

with the estimate $\hat{\Sigma}$ being comprised of all these elements.[14] We then use this to form the estimator $\hat{\mathbf{\Omega}}$ by creating a block diagonal matrix with the $\hat{\Sigma}$ matrices along the diagonal.[15] As the number of time points increases, $\hat{\Sigma}$ becomes an increasingly better estimator of Σ. We cannot, however, assess the finite sample performance of PCSEs by analytic methods. Thus we shall have to evaluate them with the same Monte Carlo experiments we use to evaluate Parks.

MONTE CARLO ANALYSIS

We have argued that the Parks method may not perform well in correcting for a variety of TSCS complications and, in particular, may lead to substantial underestimates of variability in finite samples. We have also argued that using OLS with PCSEs is a reasonable estimation strategy. We designed a series of Monte Carlo experiments to assess Parks, PCSEs, and OLS in the TSCS context. As argued, we can consider the two components of the Parks GLS "correction" separately.

Design of the Experiments

All experiments used simulated data that were generated to mimic some property of TSCS data. The setup of each simulation was similar. For a given N and T, observations on an independent variable, $x_{i,t}$, $(i = 1, \ldots, N;\ t = 1, \ldots, T)$, were generated as random draws from a zero-mean normal distribution. Experiments were run with various combination of N

and T chosen to reflect values typically found in cross-national panel models (see Table 1).

For each experiment, we generated, using GAUSS, one thousand replicates of the NT error terms ($\epsilon_{i,t}^{(l)}$, $i = 1, \ldots, N$; $t = 1, \ldots, T$; $l = 1, \ldots, 1000$) according to the model being studied. (A superscript (l) denotes a specific replicate.) The errors were always generated as zero-mean NT-variate normals, with standard deviations chosen so that estimated coefficients were roughly twice their standard errors. We chose the covariance structure of the errors to mimic the property of TSCS models being examined in a given set of experiments; this structure is discussed in the context of each set of experiments.

The $x_{i,t}$ were fixed over these one thousand replications. The one thousand replicates of $\epsilon_{i,t}$—and the fixed $x_{i,t}$—were used to generate one thousand replicates of $y_{i,t}$, using

$$y_{i,t}^{(l)} = \beta_0 + \beta_1 x_{i,t} + \epsilon_{i,t}^{(l)}; i = 1, \ldots, N;$$
$$t = 1, \ldots, T; l = 1, \ldots, 1000$$

where both β_0 and β_1 were fixed at 10. We report only statistics concerning the estimation of β_1.

The OLS estimate of β_1 for replication l is referred to as $\hat{\beta}^{(l)}$; the Parks estimate for that replication is $\tilde{\beta}^{(l)}$. We are concerned with the performance of the estimated standard errors. An accurate measure of the sampling variability of each estimator is the standard deviation of the one thousand $\tilde{\beta}^{(l)}$s or $\hat{\beta}^{(l)}$s. The quality of the OLS or Parks estimates of variability can then be assessed by comparing the root mean square average of the one thousand estimated standard errors with the corresponding standard deviation of the one thousand estimates. The measure of accuracy we focus on, *overconfidence*, is the percentage by which, say, FGLS understates variability; that is

$$\text{Overconfidence} = 100 \frac{\sqrt{\sum_{l=1}^{1,000} (\tilde{\beta}^{(l)} - \bar{\tilde{\beta}})^2}}{\sqrt{\sum_{l=1}^{1,000} (\text{s.e.}(\tilde{\beta}^{(l)}))^2}}.$$

Overconfidence of 200%, for example, indicates that the true sampling variability of an estimator is, on average, twice the reported estimate of that variability.

An alternate measure of the accuracy of standard errors is the true "level" of reported "95% confidence intervals." The "95% confidence interval" for each of the one thousand replications was computed using the estimated standard errors. We can then see how often these intervals contained the true value of β. Insofar as this proportion is under 95%, estimated variability leads to overconfidence.

We were also interested in the relative efficiency of Parks and OLS. Since the true value of β_1 is known, the root mean square error of the OLS and Parks estimates of β_1 can be calculated. The relative efficiency of OLS as compared to Parks is given by

$$\text{Efficiency} = 100 \frac{\sqrt{\sum_{l=1}^{1,000} (\tilde{\beta}^{(l)} - \beta_1)^2}}{\sqrt{\sum_{l=1}^{1,000} (\hat{\beta}^{(l)} - \beta_1)^2}}.$$

Efficiency greater than 100% indicates that OLS is superior, in mean square error terms, to Parks.

Overconfidence of the Parks Correction

Correcting for Contemporaneously Correlated Errors. The first set of experiments was designed to evaluate the overconfidence of standard errors produced by the Parks FGLS transformation to eliminate contemporaneous correlation of the errors. This overconfidence is simply a function of N and T and does not depend on the actual contemporaneous correlation of the errors. We therefore generated the data as simply as possible, with the errors drawn as independent normals. The errors, in other words, were generated so as to appear spherical.[16]

The striking results of this experiment are reported in Table 2. In the worst cases, where T is exactly N, Parks is overconfident by 400% for $N = T = 10$ and by 600% for $N = T = 20$. Reported "95% confidence intervals" contain the true value of β only 30% of the time when $N = T = 10$; this falls to 20% when $N = T = 20$.

The Parks-induced overconfidence remains a serious problem as T grows in relation to N. Even in the most favorable case for Parks ($N = 10$, $T = 40$), its standard errors are 30% overconfident and reported 95% confidence intervals contain the true β only 87% of the time. In what is perhaps the most typical configuration, with $N = 15$ and $T = 30$, FGLS errors are more than 50% overconfident and reported 95% confidence intervals contain the true value of β only about 80% of the time. Readers confronted with analysis using the Parks correction for contemporaneous correlation of the errors in such a situation should substitute 2.6 for the conventional critical t-ratio of 1.68. These results show that TSCS researchers should simply avoid the Parks correction for contemporaneously correlated errors unless the ratio of the T to N is well above three, a situation not normally seen.

Correcting for Serially Correlated Errors. The Parks method first eliminates serial correlation of the errors by transforming the data, allowing for a separate transformation for each unit. We have argued that this may induce substantial overconfidence, a problem that could be eliminated by assuming that the serial correlation of the errors followed an identical process for all units. In order to test this claim of overconfidence, as well as to compare the relative efficiency of unit-specific versus common error processes, we needed to design a more complex set of experiments. The added complexity is necessary because the performance of the estimators varies with the degree of trend in the data.

The experiments generated data where the simulated errors were generated as AR1 processes with

TABLE 2

Overconfidence of Parks Correction for Contemporaneously Correlated Errors

N[a]	T[b]	OVERCONFIDENCE (%)[c]	LEVEL[d]
10	10	408	31
	15	186	70
	20	152	78
	30	131	87
	40	130	87
15	15	529	24
	20	213	63
	30	156	79
	40	138	84
20	20	631	21
	30	187	70
	40	153	81

[a] Number of cross-sectional units.

[b] Number of time points.

[c] Overconfidence $= 100 \frac{\sqrt{\sum_{l=1}^{1,000}(\hat{\beta}^{(l)}-\bar{\hat{\beta}})^2}}{\sqrt{\sum_{l=1}^{1,000}(\text{s.e.}(\hat{\beta}^{(l)}))^2}}$.

[d] Percentage of 95% confidence intervals containing β.

unit-specific ρ_i. More precisely, the errors were generated according to expression 4, where the ρ_i were set to ρ_1 for the first half of the units and ρ_2 for the second half; the separation between these two values was varied experimentally. The degree of trend in the independent variable was also experimentally manipulated. Thus the independent variables were generated according to $x_{i,t} = \delta x_{i,t-1} + \mu_{i,t}$, where the $\mu_{i,t}$ are zero-mean, independently distributed standard normal variates. Values of δ near 1 indicate strongly trending data. The experiments then proceeded as before.

Results of the experiments are presented in Table 3. Experiments with different values of N showed that the relative performance of the two estimators was sensitive only to the value of T; we therefore only show the results for $N = 15$. In addition, since the results so strongly favor estimation with a common ρ, we present only the results least favorable to that method.

The experiments show that standard errors assuming a common ρ were never far off. Even in the worst cases, where half the units had $\rho = .9$ and half had $\rho = .3$, common ρ standard errors were never more than 20% too low; more typically, the common ρ estimator of variability was within 5% of the true value.[17]

The situation was very different for the unit-specific ρ_i estimator. The overconfidence of this estimator can be substantial; this overconfidence varies with T, δ and ρ. The most important determinant of overconfidence is δ, that is, the degree of trend in the independent variable. When $\delta = .9$, the Parks standard errors understate variability by almost 100% for T less than 30 and by about 30% for T above 30. Overconfidence caused by correcting for unit-specific serial correlation disappears only when the data trend mildly and T is at least 30.

Efficiency considerations clinch the case for estimation with a common ρ. When T is under 20, estimation assuming a common ρ is always more efficient than estimation assuming a varying ρ, even when the errors were generated with two very different ρ_i. Even when T gets as large as 40, estimation assuming a common ρ remains more efficient than estimation assuming a varying ρ, unless the errors were generated with two extremely different ρ_i. Even in this extreme case, estimation assuming varying ρ_i is less than 20% more efficient than estimation assuming a common ρ. Such extreme cases are unlikely to arise in practice. These experiments clearly show that TSCS analysts should correct for serially correlated errors assuming a common ρ.[18]

Thus, while the Parks (unit-specific) correction for serial correlation causes fewer problems in terms of overconfidence than does the Parks correction for contemporaneously correlated errors, it does induce substantial overconfidence. Where the data trend strongly, as they do in many political economy studies, this overconfidence can be as much as 100%.

Ordinary Least Squares with Panel-corrected Standard Errors

The overconfidence induced by the Parks standard errors makes it unusable except in the rarest of research situations. We proposed a simpler estimator, OLS with PCSEs. How does the proposed estimator perform in the Monte Carlo experiments? We first assess the accuracy of PCSEs and then compare the efficiency of OLS estimators with those produced by the Parks method.[19]

The Accuracy of Panel-corrected Standard Errors. OLS standard errors are accurate in the presence of either panel heteroscedasticity or contemporaneous correlation of the errors if the terms in the error covariance matrix, $\mathbf{\Omega}$, are not related to the squares and cross products of the independent variables. Since we wished to study the performance of PCSEs when OLS standard errors were incorrect, we designed experiments with contemporaneously correlated and/or panel-heteroscedastic error structures that were related to the panel structure of the independent variables.

For each value of t, the N-vector $x_{i,t}$ ($i = 1, \ldots, N$) was generated as a draw from a zero-mean N-variate normal distribution. Varying degrees of heteroscedasticity were simulated by setting the variance of the first half of the units to 1 while the variance of the second half of the units was experimentally manipulated. The covariance matrix of this multivariate distribution was constructed so that all pairs of units were equally correlated, with the degree of correla-

TABLE 3

Comparison of Feasible Generalized Least Squares Corrections for Common and Varying ρ

T^a	δ^b	ρ_1^c	ρ_2^d	OVERCONFIDENCE (%)^e SINGLE ρ	OVERCONFIDENCE (%)^e VARYING ρ	EFFICIENCY (%)^f
10	.9	.9	.3	95	186	147
10	.5	.9	.3	98	124	136
10	.3	.9	.3	100	108	129
20	.9	.9	.3	117	199	129
20	.5	.9	.3	104	125	118
20	.3	.9	.3	104	111	111
30	.9	.9	.5	103	133	102
30	.9	.9	.3	100	136	93
30	.5	.9	.5	100	102	100
30	.5	.9	.3	96	101	96
30	.3	.9	.5	100	98	100
30	.3	.9	.3	97	96	96
40	.9	.9	.5	102	132	102
40	.9	.9	.3	104	125	83
40	.5	.9	.5	100	104	101
40	.5	.9	.3	99	102	94
40	.3	.9	.5	100	101	101
40	.3	.9	.3	99	98	94

[a] Number of time points; number of units fixed at 15.

[b] $x_{i,t} = \delta x_{i,t-1} + \mu_{i,t}$.

[c] Serial correlation of errors of first half of units.

[d] *Serial correlation of errors of second half of units.*

[e] Overconfidence = $100\,\dfrac{\sqrt{\Sigma_{l=1}^{1,000}(\hat{\beta}^{(l)}-\bar{\hat{\beta}})^2}}{\sqrt{\Sigma_{l=1}^{1,000}(\text{s.e.}(\hat{\beta}^{(l)}))^2}}$.

[f] Efficiency = $100\,\dfrac{\sqrt{\Sigma_{l=1}^{1,000}(\widehat{\beta_{\text{Varying}\ \rho}}^{(l)}-\beta_1)^2}}{\sqrt{\Sigma_{l=1}^{1,000}(\widehat{\beta_{\text{Common}\ \rho}}^{(l)}-\beta_1)^2}}$.
Over 100% indicates superiority of common ρ estimator.

tion also experimentally manipulated.[20] Errors were then generated so that the variances and covariances of the errors were proportional to the corresponding variances and covariances of the independent variable. The errors could therefore show panel heteroscedasticity and contemporaneous correlation, either alone or in combination.

Table 4 shows a few key results from these experiments; a more complete table is in our companion article. These experiments set $N = 15$, vary T, and allow for various combinations of heteroscedasticity[21] and contemporaneous correlation of the errors.

Panel-corrected standard errors performed excellently in these experiments. They were always within 10% of true variability, even under conditions of extremely high heteroscedasticity and contemporaneous correlation of the errors.[22] In a typical research situation, we would expect PCSEs to be off by only a very few percentage points.

Of equal importance, in the case of homoscedasticity and contemporaneously independent errors, where OLS standard errors are accurate, PCSEs performed exactly as well as the OLS standard errors. But (as expected) as the errors became less spherical, the performance of the OLS standard errors declined.[23] Thus PCSEs dominate OLS standard errors; when PCSEs are not necessary, they perform as well as the OLS standard errors, and when OLS standard errors perform poorly, PCSEs still perform well. Since PCSEs are not difficult to compute, they should replace OLS standard errors for TSCS data.[24]

The Relative Efficiency of OLS and Parks. Panel-corrected standard errors perform well and are more accurate than Parks standard errors. Parks, however, was designed to take account of the panel error structure and hence be more efficient than OLS. The combination of OLS and PCSEs can only be clearly recommended if the OLS estimates of the parameters of equation 1 are, at worst, not much less efficient than the Parks estimates. We therefore designed experiments to compare the relative efficiency of OLS and Parks.

These experiments generated the errors completely independently of the independent variable.[25] The experiments generated the errors to show contemporaneous correlation of the errors. As before, for ease of exposition we generated the errors so that all units showed the same level of contemporaneous correlation of those errors.

Results of these experiments are in Table 5. Each entry in the table represents the relative efficiency of OLS as compared to Parks, with, for example, the first entry of 102 indicating that OLS is 2% more

TABLE 4

Ordinary Least Squares and Panel-corrected Standard Errors

T[a]	HETEROSCEDASTICITY[b]	CONTEMPORANEOUS CORRELATION	OVERCONFIDENCE (%)[c] OLS	OVERCONFIDENCE (%)[c] PCSE
10	0	0	102	102
10	0	.25	135	105
10	.3	0	119	102
10	.3	.25	144	105
20	0	0	95	96
20	.3	0	113	96
20	.3	.5	231	103
30	0	0	101	101
30	0	.5	229	107
30	.3	.5	234	106
40	0	0	104	104
40	0	.5	220	105
40	.3	0	120	102
40	.3	.5	225	104

[a] Number of time points; number of units fixed at 15.
[b] Standard deviation of $1/\sigma_i$, normalized.
[c] Overconfidence $= 100 \dfrac{\sqrt{\sum_{l=1}^{1,000}(\hat{\beta}^{(l)}-\bar{\beta})^2}}{\sqrt{\sum_{l=1}^{1,000}(\text{s.e.}(\hat{\beta}^{(l)}))^2}}$.

accurate (in terms of the square root of mean squared error) in estimating β_1 than is Parks.

Ordinary least squares is, as expected, more efficient than Parks when the errors are uncorrelated (spherical). But even when the average correlation of the errors rises to .25, OLS remains slightly more efficient than Parks. Parks becomes more efficient than OLS when average contemporaneous correlations rise to .50, but this advantage is noticeable only when the number of time points is at least double the number of units. Even here, the efficiency advantage of Parks over OLS is under 20%. Only when the average contemporaneous correlation of the errors rises to .75 is the advantage of Parks marked, and then only when *T* is twice *N*.

Researchers can use the OLS residuals to compute the average contemporaneous correlation of the residuals.[26] Researchers should find OLS acceptable unless the average contemporaneous correlation is at least .50 and the time sample is quite long. We have done this calculation for a variety of TSCS data sets that were sent to us, and none of them met this condition. It is, of course, possible that some TSCS data might show extremely high contemporaneous correlation of the errors. For such data, researchers should consider alternatives to OLS, although the inaccurate standard errors of the Parks method would not make it the alternative of choice. A better strategy would be to model the cause of the high unit correlations directly, allowing whatever is causing unit errors to covary to be a variable in equation 1. But the need to do so should occur very rarely.

REANALYSIS

Hicks and Swank

We now use our methodology to reanalyze the study of Hicks and Swank (1992) and to draw some conclusions about a variety of other analyses. The underlying model assessed by Hicks and Swank, the *social democratic corporatist* model, has played an important role in the recent study of comparative political economy.[27] The Hicks and Swank study was chosen both because it exemplifies the issues we have been discussing and because the authors were kind enough to make their data available. Hicks and Swank used the Parks procedure as implemented in the computer package SHAZAM; reanalysis was done using RATS. We had no difficulty replicating the original Hicks and Swank results using RATS; all of the differences between our findings and theirs are due to changes in methodology.

Hicks and Swank are interested in the political and economic determinants of welfare spending in 18 advanced industrial societies for the 23-year period 1960–82. Here we reanalyze their "short model" containing only variables that pass a "jackknife" test (Hicks and Swank 1992, 667).[28] Their dependent variable is welfare spending as a proportion of gross domestic product. They use a variety of political, institutional, and economic independent variables. The political variables are electoral turnout and nine measures reflecting the strength of various parties: the strength of the Left, Center, and Right in the

TABLE 5

Relative Efficiency of Ordinary Least Squares Compared to Parks (%)

		CONTEMPORANEOUS CORRELATION OF THE ERRORS			
N[b]	T[c]	0	.25	.5	.75
10	10	102	100	99	97
	20	109	101	88	72
	30	112	105	90	68
	40	109	101	87	66
15	15	101	100	99	98
	20	108	102	93	84
	30	111	101	88	72
	40	111	100	83	64
20	20	102	101	100	99
	25	107	102	97	90
	30	107	100	91	80
	40	112	104	92	76

Note: Efficiency $= 100 \dfrac{\sqrt{\sum_{l=1}^{1,000}(\tilde{\beta}^{(l)}-\beta_1)^2}}{\sqrt{\sum_{l=1}^{1,000}(\hat{\beta}^{(l)}-\beta_1)^2}}$.

Over 100% indicates superiority of OLS.
[a] Number of cross-sectional units.
[b] Number of time points.

TABLE 6

Estimates of the Hicks and Swank Model of Social Security Spending in 18 Advanced Industrial Societies, 1960–82[a]

	PARKS–AR1[b]			OLS–AR1[c]			
	b	se	t	b	OLS se	PCSE	t[d]
Turnout	5.7	.75	7.6	4.1	2.6	2.5	1.6
Government							
Left	.3	.72	.5	−1.11	1.8	1.7	−.6
Center	3.0	.65	4.6	1.01	1.8	1.6	.6
Right	1.4	.64	2.1	−.12	1.9	1.7	−.1
Opposition							
Left	−6.8	1.6	−4.1	−9.8	4.1	3.8	−2.6
Center	7.1	1.2	6.0	4.5	3.3	2.8	1.6
Right	3.0	1.2	2.5	.44	4.0	3.6	.1
Left corporatism	1.8	.33	5.3	1.8	.76	1.1	1.6
State centralization	1.8	.19	9.3	1.5	.70	.54	2.9
Bureaucratic traditionalism	.95	.25	3.8	1.6	.67	.73	2.2
Center-Left	−5.4	1.21	−4.5	−6.1	3.6	3.5	−1.7
Left-Center	2.9	.90	3.3	5.6	2.8	2.6	2.2
Left-Right	4.9	.93	5.3	5.1	3.2	2.9	1.7
lnGDP	1.0	.25	4.1	.61	.99	.87	.7
Price level (×100)	5.4	.37	14.4	4.7	1.3	1.1	4.3
Aged share of population	72.0	6.1	11.7	73.0	22.0	19.0	3.9
Post-OPEC	1.5	.08	18.1	1.6	.28	.23	7.0
Constant	−2.3	1.1	−2.0	.22	.13	.11	2.0
ρ^e	.90	—	—	.94	—	—	—

Note: The dependent variable is SOCIAL SECURITY SPENDING AS % OF GDP.

[a] Hicks and Swank estimates multiplied by 100.

[b] From Hicks and Swank 1992, table 3, col. 4, correcting for both contemporaneously correlated and unit-specific AR1 errors.

[c] Correcting for AR1 errors (common ρ).

[d] Computed using PCSEs.

[e] Average of unit-specific ρ_i for Parks–AR1, single ρ for OLS–AR1.

government; three similar measures for opposition party strength; and three interaction terms between government and opposition (center governments and left opposition, left governments and center opposition, and left governments and right opposition). A factor analysis of institutional variables yields three additional explanatory factor scores: left corporatism, state centralization, and bureaucratic patrimonialism. Finally, the model includes a variety of economic and social controls: the natural log of gross domestic product, the rate of inflation, the proportion of the population that is elderly, and a post-1973 OPEC oil-shock dummy. The model is linear in parameters and variables are measured so that all effects in the model are contemporaneous; dynamics are captured by (unit-specific) serially correlated errors. The model does not contain country dummy variables.

The estimates reported by Hicks and Swank, computing with the Parks method allowing for country-specific ρ_i are in Table 6, columns 1–3, labeled "Parks-AR1." Their t-ratios are impressive, with 13 of 17 coefficients having t-ratios over four. However, our analysis shows that the Hicks and Swank standard errors may understate variability by a factor of three.

We reestimated the Hicks and Swank equation using OLS after correcting for serial correlation of the errors, assuming a common ρ. Results of this estimation are in Table 6, columns 4–7. Both the usual standard errors (OLS se) and PCSEs are reported.[29] The t-ratios reported were computed using PCSEs.

The Hicks and Swank data, transformed to eliminate serially correlated errors, showed both heteroscedasticity (standardized measure of .37) and contemporaneous correlation of the errors (average of .25). Under these conditions, PCSEs and OLS standard errors should differ. They typically did differ but only by about 10%. While we base our conclusions on the PCSEs, we would have made similar findings using OLS standard errors.[30]

Panel-corrected standard errors are, as predicted, roughly three times the standard errors obtained by Hicks and Swank.[31] The Hicks and Swank data, when estimated with corrected standard errors, are not consistent with many of their conclusions. Hicks and Swank find that their "results support expectations, strongly rejecting most null hypotheses" (1992, 665). Our reanalysis finds that of the thirteen political and institutional variables in the short model, only four show t-ratios exceeding 2.0. Hicks and Swank find that the "evidence for positive voter turnout effects is pervasive and robust" (1992, 668). Our reanalysis finds this effect to be marginally statistically insignificant. Hicks and Swank find that "significant positive estimates for social democratic corporatism, state administrative/political capabilities and traditional political legacies [are] everywhere strong and robust" (1992, 668). We find the social democratic corporatist effect to be marginally statistically insignificant, the state administrative/political capabilities effect to be moderately statistically significant and only the traditional bureaucratic legacies effect to be strongly statistically significant. Hicks and Swank find that "overall, strong evidence emerges for strategic interactions among parties" (1992, 668). We find that of the three interaction effects that pass the jackknife test, two are marginally significant and one is moderately so. Like Hicks and Swank, we do find the economic and social controls to have a generally strong impact on welfare spending, but those are not the variables of interest. While it is possible to argue about the credibility of findings that are at the margin of statistical significance, our reanalysis clearly casts doubt on the strength of the Hicks and Swank conclusions.

Other Studies

It is impossible to use the Parks method if the length of the time frame, T, is smaller than the number of units, N. Several published studies, therefore, present results that are either logically impossible to obtain or are completely a function of numerical inaccuracies. We have shown elsewhere that Alvarez, Garrett, and Lange's (1991) original results on the interaction of the strength of labor, party control, and economic outcome were simply artifacts of numerical inaccuracies in an old, unsupported, SAS procedure. Reanalysis using the methods recommended here supported their principal finding about economic growth but not their findings on unemployment or inflation (Beck et al. 1993). Several other articles listed in the lower half of Table 1 report having used the Parks method with $N > T$.[33] It is possible that these impossible results were obtained using the problematic routine. But while we cannot know how the results reported in these articles were achieved, we can be sure that they cannot have been achieved by appropriate use of the Parks method.

Turning to studies where the Parks method can in theory be useful, we reanalyzed Swank's (1992) study of the impact of politics on tax policy in 16 Organization for Economic Cooperation and Development nations observed over 20 years. With a sample this size, our simulations indicate that Swank's estimated standard errors should be overconfident by a factor of at least three; our reanalysis bore this out. The reanalysis, using OLS and PCSEs, showed that few of his coefficients, and none of his political coefficients, were significantly different from zero.

We did not reanalyze data sets used in the other Parks analyses (upper half of Table 1) and so can only use the Monte Carlo results to assess the inaccuracy of their standard errors. The findings of Hicks (1994b) about the short-run political causes of economic growth and unemployment, which are based on Parks analyses, are very problematic.[34] With observations on 17 countries over only 18 years, reported standard errors for these analyses should be overconfident by over 600%. Thus, while he reports findings with impressive t-ratios that are often near 10, correct computation of standard errors would lead to few if any rejections of the null hypothesis that none of the social democratic corporatist variables affect economic performance. Hicks concludes that "in the short run of year-to-year economic fluctuations, the social democratic corporatist (SDC) theory of economic performance is distinctly upheld for the case of economic growth" (p. 208). This is almost certainly an artifact of computing overconfident standard errors.

Su, Kamlet, and Mowery (1993) estimated models for disaggregated middle-class and defense budget categories with contemporaneously correlated errors.[35] Their most surprising finding was that party had a statistically significant effect on middle-class spending programs. This finding is based on 10 subprograms observed over 26 years. Our simulations indicate that their "significant" t-ratio of 2.5 is on the cusp of being significant at the .05 level.

Pampel's (1993) model relating fertility to cohort size and other socioeconomic variables worked with a larger time frame (36 years). His estimated standard errors are about 40% overconfident due to the correction for contemporaneously correlated errors, plus an additional factor for using unit-specific ρ_i to correct for serial correlation. The size of the latter factor is not known, because it is a function of the unknown trend in his data. Thus he should have used a critical value for his t-tests of between 2.5 and 3. Even with this higher critical value, it appears that many of his findings would remain statistically significant, although some of his findings of significant interactions between cohort size and socioeconomic variables would be overturned by the use of a more realistic critical value.

Finally, Schneider and Ingraham (1984) have an even longer time frame (59 years). With such a time frame the Parks standard errors are only slightly overconfident; none of the findings of that paper would be overturned by using more accurate standard errors. It is only with time frames as long as this study that the Parks method might prove useful. Such long time frames are exceedingly rare in the political science literature.

CONCLUSIONS

In his recent discussion of the analysis of TSCS data in comparative political economy, Hicks notes that "we should, pending information on the small sample properties of standard errors and t statistics in the Parks-Kmenta model, be wary of downward bias in standard errors and upward bias in t-statistics to the extent that $N(N - 1)/2$ approaches NT" (1994a, 186). The present article answers Hicks' request and shows clearly that the downward bias in standard errors makes the Parks technique unusable unless there are substantially more time points (T) than there are cross-sectional units (*N*). In particular, the Parks technique is extremely misleading for the types of TSCS data typically analyzed by political scientists. As a consequence, the substantive conclusions of many articles that use the Parks method to estimate TSCS models are, at best, open to doubt. We counterbalance this negative conclusion by providing a simple methodology for analyzing TSCS data.

Time-series cross-section analysts should proceed by first examining the temporal properties of their data. This can be done, as we argue elsewhere, with lagged dependent variables or, as is typically done in the cross-national panel literature and as we do here, by transforming the data to eliminate serial correlation of the errors (Beck and Katz n.d.). If researchers choose the latter route, there is no doubt that they should transform based on an estimate of the common serial correlation; researchers correcting for serial correlation only should follow a similar strategy.

Once the dynamics are accounted for or transformed away, TSCS analysts can estimate model parameters by OLS. Our Monte Carlo evidence shows that this will seldom lead to a substantial loss of efficiency for the types of TSCS data typically analyzed by political scientists. Standard errors should be calculated using PCSEs. Our Monte Carlo evidence shows that there can be no harm from using PCSEs, while, in some circumstances, they may be considerably more accurate than the usual OLS standard errors. The combination of OLS with PCSEs allows for accurate estimation of variability in the presence of panel error structures without inducing the severe problems caused by the Parks method.

Researchers who worry that their data may fall into one of the extreme cases of heteroscedasticity or contemporaneous correlation of the errors can check for these problems by examining the structure of the OLS residuals. Only if these problems are severe, and only if sample sizes are large enough, should researchers contemplate a more complicated FGLS estimation strategy. Those contemplating such a strategy must trade-off the potential advantages of FGLS against the disadvantages of inaccurate standard errors. We have not seen a TSCS data set that makes it necessary even to consider this trade-off.

Many will take comfort in our finding that the workhorse of political methodology, OLS, is superior to the more complicated GLS approach to the analysis of TSCS data. We do not argue that complicated methods will always have problems, or that OLS is in general superior to complicated methods. It is, however, critical that we learn to assess the properties of complicated estimation strategies, and in particular that we study these properties for the types of data actually analyzed, rather than for large samples observed only in "asymptopia." This is particularly true for assessments of variability, about which we often have little or no intuition. Ordinary least squares, with corrected standard errors, will not always prove to be superior to more complicated techniques, but it clearly is superior to the Parks method for analyzing the types of time-series cross-section data that are used by students of comparative politics.

Notes

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1. This method was popularized in Kmenta's text, so it is sometimes referred to as Parks-Kmenta and sometimes attributed only to Kmenta (1986, 622–25). We call it "Parks" throughout this article.

2. We have written a companion article, Beck and Katz n.d., that treats the dynamic issues of TSCS estimation in detail; it also examines estimators that correct for panel heteroscedasticity only.

3. We assume that the reader is familiar with the basics of TSCS models, as laid out in Stimson 1985 and more fully in Hsaio 1986. Stimson distinguished models that are cross-sectionally dominated from ones that are not. The former (e.g., the National Election Studies Panels) have observations on thousands of units observed a few times. While TSCS models are formally equivalent to such panel designs, the problems faced by TSCS modelers are very different from those faced by electoral panel modelers. The present article does not consider issues that arise in cross-sectionally dominated designs. While we typically call the designs we study TSCS models, in a few places it is more convenient to refer to our designs as *panels;* the two terms are used interchangeably here. Earlier versions of this article used the term *cross-national panel.* While this term correctly connotes the size of cross-section we consider, we do not wish in any way to limit the range of applications of our method to cross-national data.

4. The exogenous variables may contain unit-specific dummy variables, allowing intercepts to vary by unit. Such a model is called a *fixed effects* model. Fixed effects present no special problems for TSCS models, because the number of unit-specific dummy variables required is not large. We do not consider random effects models, which are heavily used in cross-sectionally dominated panel models but are not important for TSCS work. We show in Beck and Katz n.d. that it is easy to include lagged dependent variables in expression 1.

5. That is, the data are ordered so that the second observation is the observation on unit 1 for the second time period and, in general, the observation following unit i for time period t is the observation for unit i for time period $t + 1$ (or, following the last observation on unit i, it is the first observation on unit $i + 1$).

6. We can state the various "panel error" assumptions symbolically as:

Panel Heteroscedasticity. $E(\epsilon_{i,t}^2) \neq E(\epsilon_{j,t}^2)$, but $E(\epsilon_{i,t}^2) = E(\epsilon_{i,t'}^2)$, so we can write $E(\epsilon_{i,t}^2) = \sigma_i^2$.

Contemporaneously Correlated Errors. $E(\epsilon_{i,t}\epsilon_{j,t}) = E(\epsilon_{i,t'}\epsilon_{j,t'}) \neq 0$, but $E(\epsilon_{i,t}\epsilon_{j,t'}) = 0$, so we can write $E(\epsilon_{i,t}\epsilon_{j,t}) = \sigma_{ij}$, with all other covariances being zero.

Unit-specific Serially Correlated Errors. $\epsilon_{i,t} = \rho_i \epsilon_{i,t-1} + \nu_{i,t}$, where the ν are incoming "shocks" that are *temporally* independent, identically distributed, zero-mean random variables.

Common Serially Correlated Errors. $\epsilon_{i,t} = \rho \epsilon_{i,t-1} + \nu_{i,t}$, where the $\nu_{i,t}$ are incoming "shocks."

7. The FGLS method to correct for only panel heteroscedasticity is panel-weighted least squares, which is incorporated in Stimson's (1985) GLS–ARMA technique. The FGLS correction for panel heteroscedasticity is not subject to the extreme problems of the correction for contemporaneously correlated errors so we do not further consider it here. Those contemplating using this technique should see Beck and Katz n.d.

8. The same method may be used with a common serial correlation process, but the Parks method allows for unit-specific serial correlations.

9. Analysts modeling temporal dynamics with a lagged dependent variable need only use the second transformation. Other analysts use only the first stage of the Parks method, correcting only for unit-specific serial correlation. As we shall see, the use of either transformation alone leads to serious underestimates of variability.

10. $\otimes$ is the Kronecker product. For example, if $N = 2$ and $T = 3$, the variance covariance matrix of the errors is

$$\mathbf{\Omega} = \begin{pmatrix} \sigma_1^2 & 0 & 0 & \sigma_{12} & 0 & 0 \\ 0 & \sigma_1^2 & 0 & 0 & \sigma_{12} & 0 \\ 0 & 0 & \sigma_1^2 & 0 & 0 & \sigma_{12} \\ \sigma_{12} & 0 & 0 & \sigma_2^2 & 0 & 0 \\ 0 & \sigma_{12} & 0 & 0 & \sigma_2^2 & 0 \\ 0 & 0 & \sigma_{12} & 0 & 0 & \sigma_2^2 \end{pmatrix}.$$

11. Feasible generalized least squares requires that $\hat{\Sigma}$ be invertible. It is an $N \times N$ matrix, but its rank is, at most, the lesser of T and N. Thus it is not invertible unless T is at least as large as N.

12. The OLS standard errors will not be consistent. The degree of inaccuracy is a complicated function of the relationship between the $\mathbf{X'X}$ matrix and the variances and covariances of the error process. If these are only slightly related then the OLS standard errors will only be slightly incorrect. This is shown clearly for the cross-sectional case by White (1980). One consequence of this is that it not possible to infer the overconfidence of OLS standard errors as a simple function of sample size.

13. We use this term to distinguish our proposed estimate of variability from White's (1980) heteroscedasticity-consistent standard errors. Many standard packages, such as RATS or SHAZAM compute robust standard errors based on White's method; none of these take into account the panel structure of the errors, even within a panel estimation module. Our Monte Carlo results, presented in Beck and Katz n.d., show that researchers should not use robust standard errors calculated by typical computer packages to approximate PCSEs. It should also be stressed that our use of the term *panel-corrected* indicates that the standard errors computed will not be inaccurate due to the panel structure of the data. Other pathologies, such as a time-varying error structure, may indeed cause PCSEs to be inaccurate. In this regard, PCSEs are no better, but no worse, than any other estimator proposed for TSCS data.

14. We are using the maximum likelihood, rather than the unbiased, estimate of Σ. The unbiased estimate would be obtained by dividing by $T - k$. It is not clear which method is superior. In our simulations, we use only one independent variable, so the difference between the two estimators is small.

15. In symbols, if $\mathbf{E}$ denotes the $T \times N$ matrix of the OLS residuals, we can estimate Σ by

$$\hat{\Sigma} = \frac{(\mathbf{E'E})}{T}$$

and hence estimate $\mathbf{\Omega}$ by

$$\hat{\mathbf{\Omega}} = \frac{(\mathbf{E'E})}{T} \otimes \mathbf{I}_T,$$

where $\otimes$ is the Kronecker product. Panel-corrected standard errors are thus computed by taking the square root of the diagonal elements of

$$(\mathbf{X'X})^{-1}\mathbf{X'}\left(\frac{\mathbf{E'E}}{T} \otimes \mathbf{I}_T\right)\mathbf{X}(\mathbf{X'X})^{-1}.$$

16. We examined the overconfidence of the Parks standard errors in many more complicated contexts. Our findings about the overconfidence of the Parks standard errors in those experiments are identical, to within the small sampling error of the experiments, to what is reported in Table 2.

17. As in all our experiments, we are assessing the estimation of β_1 and its variability.

18. Some TSCS analysts do not use the Parks method but do correct for serially correlated errors using unit-specific ρ_i (e.g., Levobic 1994; Pollins 1989; Rosh 1988). The simulation results apply equally to this method. Correcting for unit-specific serial correlation leads to substantial overconfidence, whether used alone or as part of the Parks method.

19. We deal only with the performance of these estimators assuming that serial correlation has already been eliminated.

20. The contemporaneous correlation of the errors matrix was assumed to take this simple form for ease of exposition. None of the experimental results are artifacts of this simple form, since the various estimation techniques allow for freely varying contemporaneous correlations.

21. We have seen no textbook measure of the degree of panel heteroscedasticity. It is measured for Table 4 by the standard deviation of the normalized weights that would be used in panel-weighted least squares. The variance of the i^{th} unit is σ_i^2. Let $w_i = 1/\sigma_i$. We define *standardized heteroscedasticity* as the standard deviation of the $w_i/\bar{w}$.

22. This result is based on both Table 4 and results reported in Beck and Katz n.d.

23. Again, we designed the experiments where OLS performance would be degraded. In practical situations the OLS standard errors *may* perform well.

24. The code to compute PCSEs in RATS is available via ftp from weber.ucsd.edu.

25. The inefficiency of OLS in the presence of nonspherical errors does not depend on the relationship of the error process and the independent variables.

26. This statistic is computed in our RATS program.

27. This importance is best seen in the variety of papers in Janoski and Hicks 1994.

28. For definition and operationalization of these variables, see Hicks and Swank 1992, 663–64. We received data only on variables contained in the short model. Results from this model are similar to other results reported by Hicks and Swank.

29. Panel-corrected standard errors were computed after the data were transformed to eliminate serial correlation.

30. This is not an argument for using OLS standard errors. It is difficult to know, in advance, whether OLS standard errors are accurate, while we are confident about the accuracy of PCSEs. The only way we know that OLS standard errors are accurate for this reanalysis is that they are close to the estimated PCSEs. Since there is no reason for TSCS researchers to compute possibly misleading OLS standard errors, we focus on the PCSEs here.

31. We also estimated the Hicks and Swank model using our methods but allowing for unit-specific ρ_i. These estimates (not reported here) had, as predicted, PCSEs that were between 25% and 50% smaller than the PCSEs computed under the assumption of a common ρ. If we compare PCSEs to Parks standard errors when both correct for serially correlated errors similarly, we find that the PCSEs are, as predicted, about two-and-a-half times larger than the corresponding Parks standard errors.

32. Hicks and Swank appear to equate a strong effect with

one having a large t-ratio. We therefore assess their findings using this standard.

33. As noted in that table, some of these studies used Parks in addition to other estimation methods. We have no findings on the conclusions based on other methods.

34. His analysis of the long run does not use Parks and so is not discussed here.

35. This study uses a "seemingly unrelated regressions" methodology which is identical to the FGLS correction for contemporaneously correlated errors.

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COALITION TERMINATION AND THE STRATEGIC TIMING OF PARLIAMENTARY ELECTIONS

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Cabinet coalitions in multiparty parliamentary democracies lead a precarious existence. Legislative majorities can typically dismiss the cabinet at will and can sometimes force early elections through parliamentary dissolution. Since coalition termination can have substantial political consequences, it is important to understand when and why such decisions are made. To this end, we develop a model of coalition bargaining in a legislature with dismissal and dissolution powers. We use the model to identify necessary and sufficient conditions for both coalition termination and parliamentary dissolution. In contrast to several widely held maxims, we find that coalition terminations need not be the automatic consequence of exogenous shocks. Nor do opportunistic parties with favorable electoral prospects always dissolve parliament to enhance their power. Instead, decisions to terminate coalitions or call new elections result from party leaders' rational responses to the constraints of legislative and electoral institutions and the anticipated feelings of the electorate.

Coalition governments in parliamentary democracies lead a precarious existence. Typically, they can fall on any given day, sometimes with little or no warning. The circumstances surrounding coalition termination vary greatly, occasionally producing great drama. Some politicians are forced from their cabinet offices in a daze, never knowing what hit them. Others choose their date of departure and leave with smirks on their faces. Some alliances go down in fiery spectacles of backroom infighting and betrayal, whereas others take their struggles "to the country" and leave the judgment to the voters.

Why do governments fall at the precise moments that they do? Quite often, the causes of coalition terminations seem obvious in one sense—as when they follow a dissolution of parliament or a legislative vote of no confidence. "Explanations" of this kind direct our attention to two important and common legislative powers. In most parliamentary democracies, simple legislative majorities have *dismissal power,* that is, the power to recall the cabinet at any time. In many parliamentary democracies, legislative majorities also have *dissolution power,* that is, the power to dissolve parliament and force early elections. Once we recognize the availability of such powers, the more interesting questions become when and why parliamentarians use them.

We shall explain the causes and consequences of coalition termination in a legislature endowed with dismissal and dissolution powers. We pursue these questions because decisions to terminate coalitions, call new elections, or leave the government as it is, constitute dramatic events with nonobvious causes and politically important consequences. Such consequences may be direct or indirect. The best-known direct consequence is the sizable reallocation of legislative power that can follow a change in government. Indirect consequences include the effects of commonly held beliefs about the likelihood and aftermath of an upcoming termination on the present bargaining powers and negotiating strategies of forward-looking parliamentarians. All else equal, participants in a seemingly viable government may have much greater bargaining power than those whose partnership is expected to collapse soon. If such beliefs are sufficiently common or their implications large enough, then they can affect parliamentary decision making decisively and long before the cabinet's predicted demise.

To identify causes and consequences of coalition termination, we develop a model of parliamentary bargaining in the presence of a preexisting governing coalition. In the model, an external event gives rise to commonly held expectations concerning the outcome of a potential election. We use the model to identify necessary and sufficient conditions for three collectively exhaustive types of outcomes: (1) coalition termination followed by a nonelectoral reallocation of power (either reallocations between existing coalition members or reallocations involving a new set of coalition members); (2) coalition termination followed by parliamentary dissolution and new elections; and (3) no coalition termination (i.e., the governing cabinet coalition survives the crisis completely intact). The predominant theme of our results is that coalition terminations are not automatic responses to external events. Instead, the causes and consequences of coalition terminations are reasoned and negotiated responses to a variety of changing circumstances. Furthermore, we find that popular opinion, strategic interaction, and institutional features of both legislative and electoral institutions each have systematic effects on the dynamics of coalition termination.

What value such insights provide is partially revealed by three conclusions, each of which allows us to reinterpret a widely held maxim concerning cabinet formation or termination. Our first conclusion suggests thinking anew about the maxim that "parties terminate cabinets when they expect electoral gains" (e.g., Grofman and van Roozendaal 1994, 158). We conclude that *favorable electoral prospects are neither a necessary nor a sufficient condition for either coalition termination or parliamentary dissolution*. Rather, the anticipation of good electoral fortunes gives a party a bargaining chip that it can exploit by either renegotiating the balance of power within an existing coalition, forging a more attractive coalition with new partners, forcing dissolution and new elections, or protecting the existing cabinet.

Our second conclusion amends the maxim that in three-party legislatures, "the governing coalition will comprise the largest and smallest legislative parties" (Austen-Smith and Banks 1988; Baron 1991). We conclude that *the bargaining advantages often attributed to a party's size are also the result of various other factors*, such as its electoral prospects or the policy and office-driven gains from trade that it can offer potential coalition partners. So while a political party's size and bargaining power can be positively correlated, we identify a wide range of circumstances under which they are not.

The third affected maxim is that governing coalitions are toppled by exogenous shocks that are "random and unpredictable events" (e.g., Browne, Frendreis, and Gleiber 1986; Cioffi-Revilla 1984). We conclude that *whether a particular crisis leads to both coalition termination and parliamentary dissolution, just coalition termination, or neither depends heavily on the electoral cycle*. For example, the same external event that causes no dissolution early in a parliament's term could well do so later.

In sum, our research provides a new way to understand parliamentary decision making in regimes where coalitions can be terminated at almost any time. It also proposes a new way to think about the relationship between legislative institutions, electoral considerations, and parliamentary decisions.

We begin our analysis by describing two important, but unremarkable, coalition terminations. These cases' shared characteristics are emblematic of factors that generally influence coalition termination. They provide the empirical grounding for our theoretical analysis. Then we briefly evaluate the existing literature on the causes of coalition termination and stockpile the wisdom it imparts. Next, we develop a noncooperative, game-theoretic parliamentary bargaining model and use it to identify the causes and consequences of coalition termination. We supplement our results with a short series of simple numerical examples. Finally, we offer concluding remarks and, in the Appendix, provide a series of proofs.

A TALE OF TWO COALITIONS[1]

We aim to facilitate a general understanding of coalition termination's dynamics by building a theoretical model upon empirically verifiable premises. To motivate our premises, we briefly chronicle two actual coalition crises. What makes these crises particularly illustrative for our purposes is that they represent milestones in the political histories of their respective countries. Their real-world significance is therefore indisputable, and the events are relatively well known. Yet these cases are by no means exceptional. We believe they are representative of many similar events.

Please note that each case contains five features that are common to coalition crises and serve as the building blocks of our own theoretical model. First, *coalition crises typically arise with changes in the political environment, and specifically when party leaders gain new information*. Party leaders are more likely to terminate coalitions or call early elections in the wake of major unexpected events beyond their control. But what makes such events critical is the new expectations they convey. Second, *coalition crises can produce any one of a number of possible outcomes*. Sometimes, the result is simply a cabinet reshuffle, a renegotiation of the "contract" between the existing coalition members. At other times, a new coalition may form between parties that were not previously partners. One extreme resolution of a coalitional crisis is, perhaps, the best known—the decision to dissolve parliament and call early elections. At the other extreme is the possibility that the crisis passes with the initial coalition and parliament intact. Third, *intensive party deliberations and negotiations often precede coalition termination*. That is, coalition realignment does not typically take the form of an immediate response to an exogenous shock. Instead, coalition bargaining is often protracted and hard-nosed. Fourth, *parliamentarians often conduct such negotiations in the shadow of elections*. That is, the strategies of party leaders are critically influenced by their anticipation of elections and by their expectations concerning voter preferences. Fifth and finally, *party leaders often pay a price when they abandon their previous commitments*. For example, politicians often encounter resistance from their own parties when they advocate changing coalition partners.

Germany, 1982

The seventeenth of September 1982 marked the most complete turnover in the German government since World War II. After 13 years of coalition government, the leadership of the Free Democratic party (FDP) decided to terminate its cooperation with the Social Democrats (SPD). An economic downturn had put the coalition partners at odds and lessened the value of the alliance to the FDP. With a ballooning budget deficit, the gulf between the FDP's commitment to fiscal discipline and the Social Democrats' Keynesian policies had gradually led to coalitional friction. At the same time, the coalition parties faced a new electoral challenge in the Green party, which had ridden the crest of peace protests and heated debates over the death of the German forests to electoral success. The Greens had drawn more than 5% of the vote in recent state elections (Bremen and Baden-Württemberg), success that if replicated at the national level, would land them in the Bundestag (the German parliament) for the first time.

The Green success stood in direct contrast to the FDP's electoral fortunes. The FDP had lost support in five of the six state elections held since 1979 and by 1982 was out of government in all but two states, an unprecedented low. Opinion polls showed a steady decline in the proportion of German voters intending to vote for the FDP. From 12% at the beginning of the 1981, this figure had dropped to about 7% by the middle of 1982. One poll in the summer of 1982 put FDP support as low as 5.3% (Søe 1985, 151). If this trend were to continue, the Free Democrats would be in danger of falling below the 5% threshold in the next scheduled Bundestag election in 1984. These grim forecasts, combined with the Green ascendancy, threatened to end the pivotal role that the FDP had played in virtually every postwar government.

The Free Democrats had several difficult choices to make. One option was to leave the coalition with the SDP in favor of one with the Christian Democrats (CDU/CSU). Some FDP leaders felt that such a switch would help their electoral standing in the longer run. Other elements within the FDP feared the electoral repercussions of "betraying" the commitment to the SDP, on which the party had contested the previous Bundestag election. Polls showed most FDP voters opposed to any change of partners, and Free Democrats committed to the SPD–FDP coalition felt that any change of coalition should be approved by the voters ex ante. While some FDP members favored asking President Carstens to dissolve the Bundestag and hold early elections, others, conscious of the FDP's declining electoral fortunes, foresaw early elections driving the party out of parliament.

The conflict came to a head when FDP economics minister Otto von Lambsdorff called for austerity measures affecting social services, only to be publicly rebuked by Schmidt and other SPD officials (Søe 1985; 1982 *Keesing's Record*, 31,774). On 17 September, after Schmidt rejected Lambsdorff's proposals, all four FDP cabinet ministers announced their resignation. Schmidt then allowed his cabinet to be voted down and unsuccessfully sought a parliamentary dissolution. Instead, on 1 October Helmut Kohl's CDU/CSU–FDP coalition was voted into office in the first successful application of Germany's constructive vote of no confidence. Several months later, Kohl engineered a parliamentary dissolution, leading to elections in March 1983.

For the FDP, the short-term fallout of coalition change was severe. Günter Verheugen, the party's secretary general, resigned in protest. Thousands of activists and several prominent leaders (including Verheugen) then left the party, which failed miserably in three state elections between late September and December of 1982. However, the six-month delay of the Bundestag election appears to have allowed the Free Democrats to recoup some of their public support. While the FDP, as expected, lost ground electorally, it did surpass the 5% of the March vote necessary to survive as a parliamentary party and coalition partner (see Poguntke 1993).

Ireland, 1987

A few years later, an era in Irish coalition politics came to an end. On 20 January 1987, the Irish Labour party withdrew from the two-party coalition that had governed Ireland for the major part of the period since 1973. Taoiseach (Prime Minister) Garret FitzGerald of the Fine Gael, reduced to the head of a minority cabinet, promptly requested a dissolution of the Dail (the Irish parliament).

Labour's withdrawal from the third Fine Gael–Labour coalition had been long in the making. Though the two parties wasted little time in reaching agreement after the November 1982 election, the coalition soon encountered rough waters. Lingering economic recession in the mid-1980s led to increased unemployment, emigration, and industrial relations confrontations. FitzGerald's neoliberal economic policies succeeded in bringing down inflation but were unpopular among working-class voters (Girvin 1987). In addition, the government struggled with touchy issues such as abortion and divorce.[2]

The emergence of a new political party, the Progressive Democrats (PD), complicated the electoral picture. The party, founded in 1985 as a splinter group from Fianna Fail, quickly surpassed Labour in the polls to become Ireland's third largest party. Somewhat surprisingly, the PD proved to draw more support from disaffected Fine Gael voters than from Fianna Fail. Yet the problems of the coalition were particularly visited on the Labour party. From 9.4% of the vote in the November 1982 election, opinion polls showed support for the party slipping to 7–8% by the end of 1984 and all the way down to 4% by 1986 (Marsh and Mitchell 1992, 14).

In response to this alarming slippage, Labour party leaders attempted to "toughen up" their bargaining position vis-à-vis Fine Gael. Whereas their efforts yielded some policy victories, Labour leaders failed to improve their standing with the electorate and came under increasing pressure to resign from the coalition. Labour leader Dick Spring originally favored a continuation of the coalition with Fine Gael, but his position within the party gradually eroded. A Labour party electoral commission, reporting in September 1985, identified coalition participation as the greatest cause of the party's decline and advocated going into opposition. The alternative, it was suggested, "may be no party at all" (Marsh and Mitchell 1992, 15).

There was ambivalence about the coalition within the Fine Gael as well. While Prime Minister FitzGerald hoped to keep his coalition alive until a June 1987 election, he realized that Labour's government participation made it difficult to reach the budgetary goals on which he had staked much of the party's reputation. To shore up the party's shaky support, he hoped to fight the next election on the theme of fiscal integrity. At the 1986 party conference, he therefore left the door open for an abandonment of the coalition in the next parliament (Girvin 1987, 9–10).

A cabinet reshuffle in February 1986 failed to infuse any new blood into the coalition, and disgruntled backbenchers defected from both coalition parties. At its annual conference in November, the Labour party adopted a resolution that ruled out participation in any coalition after the next election (1987 *Keesing's Record*, 35,084). That the main opposition party, Fianna Fail, steadfastly maintained a "no coalition" policy implied that no other option was feasible.[3] In response to growing internal pressure and the unprecedented unpopularity of the coalition, Spring disowned the coalition in September, declaring that he looked forward to a life in opposition.

Four months later, Labour found a rationale for withdrawal in the government's proposed cuts in social welfare, health, and education (Farrell and Farrell 1987; Marsh and Mitchell 1992, 16). These measures were critical to both parties' electoral credibility. The fact that the Fine Gael had forced these measures on the cabinet despite Labour opposition set in motion the events that led to Labour's 20 January withdrawal. The subsequent elections resulted in losses for both Fine Gael and Labour, while Fianna Fail captured the executive branch.

COALITION TERMINATION AND ITS CAUSES

As coalition terminations go, neither the German or Irish case is unusual. However (and despite their strong similarities), the existing literature on coalition government does not provide us with a general understanding of why parliaments resolve coalition crises in such distinct manners.

To facilitate such an understanding, we follow a respected list of political scientists who have long recognized the importance of executive stability in democracies and have sought to identify its determinants (Bryce 1921; Lowell 1896; Powell 1982). Over time, this literature has branched off into three distinct schools. One venerable tradition seeks the causes of coalition termination in the *structural attributes* of the coalition itself (Blondel 1968; Duverger 1954; Strøm 1985; Warwick 1979) or the larger regime in which coalition politics is played out (Grofman 1989; Lijphart 1984; Lowell 1896; Powell 1982). A second and more recent literature stresses the destabilizing effects of exogenous *critical events* (Browne, Frendreis, and Gleiber 1986; Cioffi-Revilla 1984; King et al. 1990; Warwick 1992a; Warwick and Easton 1992).[4] The final and still incipient tradition in the study of cabinet stability grew out of the structural attributes tradition. It focuses on *strategic interaction* between (and possibly within) parties as the key to coalition dynamics (Austen-Smith and Banks 1988, 1990; Baron 1989, 1991; Dodd 1976; Laver and Shepsle 1990; Riker 1962; Schofield 1992). What distinguishes work in this tradition is the employment of game-theoretic solution concepts as a guide to coalition stability. The analytical and empirical application of such tools to the question of coalition termination was first seriously pursued by Dodd (1976).

We build on the most promising lessons of all three approaches. For instance, the strategic interaction school inspires us to develop a rigorous and plausible behavioral foundation for the study of coalition termination. Adopting such an approach requires that we explain coalition termination as a result of the deliberate and often complex bargaining that goes on between party leaders. It also allows us to satisfy the desire of King and his colleagues, who "agree with Browne, Frendreis and Gleiber (1988, 934) that there is as yet no fully developed micropolitical theory of cabinet durability within the rational choice tradition" and "agree with Strøm that such an approach is essential to an understanding of cabinet durability" (1990, 868–69).

We draw from the structural attributes tradition the insight that institutions matter. As Laver and Schofield stress: "A single institutional feature can make two otherwise similar coalition systems as different as chalk and cheese. Particular constitutional constraints, the German constructive vote of no confidence for example, were even designed with the specific intention of having an impact upon the politics of government formation. To ignore such matters would clearly be to stick our heads in the sand" (1990, 214).

We believe that the institutions most likely to affect coalition termination are those that most directly impinge on party bargaining over executive coalitions and legislative dissolution. Dismissal and dissolution powers are good examples of such institutions, as are electoral laws and party rules that affect the costs of negotiations and electioneering. Therefore, our theoretical model explicitly defines not only dismissal and dissolution powers but also the rules that enable parties to make or accept deals concerning coalition participation.

While we draw from the "critical events" perspective as well, our approach is also a critique of this literature. Critical events scholars teach us that we cannot anticipate the entire political life of an executive coalition at its inception: from time to time, shocks occur that affect the outlook of the governing parties. As Strøm have previously noted, however, "an adequate critical events theory of cabinet dissolution has to meet the following criteria: events must (1) enter the utility calculations of the players in the governmental game; (2) be defined and observed independently of government resignations; and (3) be allowed to vary in their (a) exogeneity and (b) sufficiency as causes of government resignations" (1988, 929).

In other words, we need to think carefully about what makes events "critical" and how they affect coalition bargaining. Our problem with the critical events approach is that it does not authoritatively describe the specific nature of critical events. In fact, a key point often made by critical events theorists is that such events are random and unpredictable.[5]

By contrast, we believe that events such as wars, scandals, and economic shocks are not inherently critical. Instead, events *become* critical through their effects on parliamentary bargaining. Thus what makes an event critical is the behavioral response it occasions among the bargaining parties. To put it bluntly, potentially critical events are meaningful only if they affect politicians' abilities to achieve their legislative and electoral goals.

In sum, common features of actual coalition crises and the existing literature on coalition dynamics lead us to propose a theory based on the premise that strategic behavior, institutional constraints, electoral motivations, and external events could figure prominently among the causes of coalition termination. The most novel feature of our theory is the inclusion of electoral motivations. Up to now, such motivations have played only a very modest role in the theoretical literature on coalition termination.[6] The previous absence of such motivations was noted by Laver and Schofield, who stated that "the problem for coalition theory, therefore, is that it has up until now been essentially static, assuming implicitly that politicians do not look forward to the next election when they bargain" (1990, 59). The benefit of including such motivations will soon become manifest.

THE MODEL

We identify causes and consequences of coalition termination by modeling coalition bargaining, in a parliament with dismissal and dissolution powers, as a game between three unitary actors called *parties*.[7] We develop a three-party model, as opposed to an n-party model, because it provides the simplest formal framework for examining the bargaining dynamics of coalition government. For expositional simplicity, we describe the case where all players share a common knowledge.[8]

The game begins at some point during parliament's term, after an initial governing coalition has been installed and soon after a potentially critical shift in public opinion occurs.[9] During the game, parties react to this "event" by either maintaining the existing coalition, negotiating a new one, or dissolving the legislature and calling new elections. We use the *subgame perfect Nash equilibrium* solution concept to draw deductively valid conclusions about party actions and bargaining outcomes (for a description of this concept, see Ordeshook 1992, 86–88).

We shall now provide a complete description of the game. For simplicity, we first describe the parties and their objectives absent a potentially critical event. We then describe the opportunities and new incentives that the event provides.

Parties and Their Objectives in the Absence of an Event

We call the three parties the *first party*, the *second party*, and the *out-party*. Each party's name indicates its relationship to the initial governing coalition. We describe the case where the first and second parties are members of the initial governing coalition, the out-party is not, and any two parties can form a new majority coalition.[10] The only substantive distinction between the first and second parties, upon which none of our results depend, is that the first party reacts to the event before the second. More importantly, we do not assume that a party's name necessarily indicates its relative size. Thus neither the first nor the second party need be the largest.

Absent a potentially critical event, each party's objective is to maximize the value it derives from its role in parliament. We make three basic assumptions about these objectives:

Assumption 1. *Parties care about controlling seats in parliament.* Let $s_i \in \mathfrak{R}^+$ be the subjective value to party i of the seats that it controls, where $i \in \{1,2,o\}$, the subscript 1 refers to the first party, the subscript 2 refers to the second party, and the subscript o refers to the out-party. Since elections that determine seat shares must occur before a parliament's term commences, and since the beginning of the game occurs after such commencement, we assume that the initial values of the s_i parameters, as well as the initial values of all other game-relevant parameters, are determined exogenous to (prior to) the play of this game. For expositional simplicity, we describe the case where the subjective value of seats to party i is equivalent to the percentage of seats that party i holds ($s_i \in [0, 1]$, $\Sigma_{i \in \{1,2,o\}} s_i = 1$).

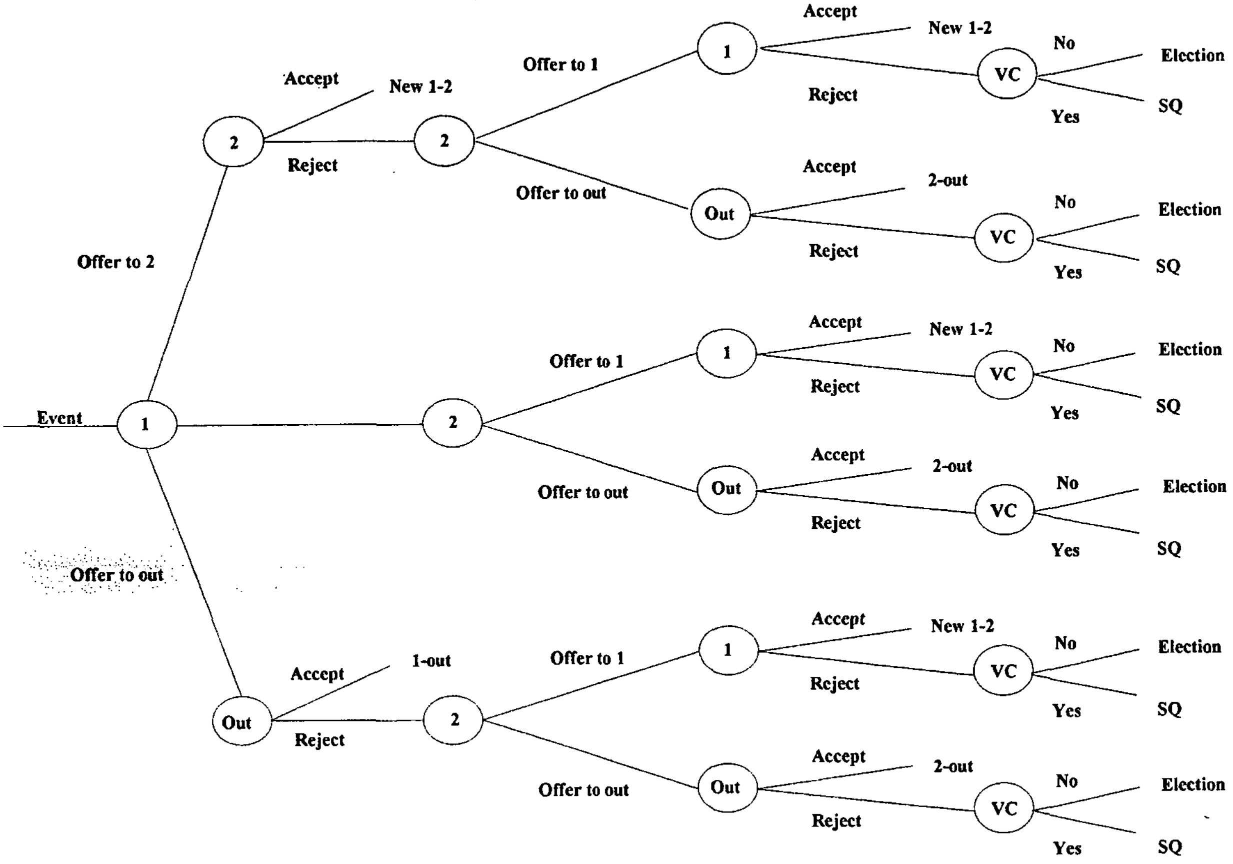
Assumption 2. *Parties value power within a governing coalition.* $c_i \in \mathfrak{R}$, $\Sigma_{i \in \{1,2,o\}}\, c_i = 1$, denotes party i's share of power in the initial governing coalition; where $c_1 + c_2 = 1$ and $c_o = 0$. c_i represents the currencies—such as portfolios (i.e., cabinet appointments) or patronage—by which the power from governing is derived, held, and transferred.[11] If we think of what a governing coalition does as dividing a valuable pie, then c_i represents the size of the pie slice that party i receives.

Assumption 3. *Parties can value some potential coalition partners more than others.* $g_i^j \in \mathfrak{R}^+$ represents the value to party i that a coalition with party j could produce. $g_j^i \in \mathfrak{R}^+$ represents the value to party j of the same coalition, and g_i^j need not equal g_j^i. In other words, if we think of one of the things that a governing coalition does as making (and then dividing) a valuable pie, then g_i^j represents the common expectation of the *total* (pre-division) value to party i of the pie that it and party j would make. g_j^i represents the value of the same undivided pie to party j.

We use g_i^j to represent the benefits of membership in a particular governing coalition. Such benefits derive from the similarity in policy preferences and/or the complementarity of office preferences among coalition members. For instance, a coalition containing parties with similar policy agendas is likely to generate greater utility for its members (all else constant) than would a coalition containing parties with

FIGURE 1

The Model's Extensive Form (Game Tree)


Event
1
Offer to 2
2
Accept
New 1-2
Reject
2
Offer to 1
1
Accept
New 1-2
Reject
VC
No
Election
Yes
SQ
Offer to out
Out
Accept
2-out
Reject
VC
No
Election
Yes
SQ
2
Offer to 1
1
Accept
New 1-2
Reject
VC
No
Election
Yes
SQ
Offer to out
Out
Accept
2-out
Reject
VC
No
Election
Yes
SQ
Offer to out
Out
Accept
1-out
Reject
2
Offer to 1
1
Accept
New 1-2
Reject
VC
No
Election
Yes
SQ
Offer to out
Out
Accept
2-out
Reject
VC
No
Election
Yes
SQ

1, 2 and Out = the names of the parties.
1-Out = a coalition between the First and Out parties.
2-Out = a coalition between the Second and Out parties.
New 1-2 = original coalition survives, power is reallocated.
VC = Vote of Confidence—means a dissolution and new elections if it fails.
SQ = Status Quo—original government survives with no power reallocation.

conflicting policy agendas. Equivalently, a coalition of parties with different preferences over cabinet appointments can have greater value to its members than one in which all parties covet the same portfolios. One advantage of our coalition-specific value concept is that we can assume that parties are goal-oriented without making a general (and controversial) assumption about the extent to which parties are interested in patronage, policy outcomes, or the prestige of holding office.[12] $c_i \times g_i^j$ is the utility that a coalition with party j offers party i.[13]

In sum, we assume that party i's utility from its role in parliament can generally be thought of as a function $F_i(s_i, c_i \times g_i^j)$.[14] Because we want parsimoniously to highlight the distinct effects of party size, coalition partner value, and governing power on coalition termination, we describe our model using a simple example of such a function: the case where party i derives utility $s_i + (c_i \times g_i^j)$ from its role in government.[15]

Party Objectives and Opportunities in the Presence of an Event

Having just described the benefits that parties derive from governing, we now describe a sequence of events that may lead the parties to cast these benefits aside. We describe this sequence of events as a game that has two periods: period 1 (the present), in which parties take actions, and period 2 (the future), in which parties realize the consequences of their period 1 actions. Figure 1 depicts the extensive form of the first period, which begins at some point during a parliament's term, after an initial governing coalition has been installed and soon after a potentially critical event occurs.

A Potentially Critical Event

An event (represented by a poll that credibly signals public opinion or a set of shared past experiences that informs electoral expectations) provides all parties with information about what would happen to them if an election were to be held in period 1. What this event reveals is $b_i \in \mathfrak{R}$, the expected utility of a period 1 election to party *i*. b_i represents all of the game-relevant information about party *i*'s postelection well-being.[16]

While elections can provide benefits, calling them can also impose several types of costs. There are *opportunity costs* to calling elections, (e.g., the forfeiture of the policy-making opportunities or rent collection opportunities made possible by holding valuable offices) and *transactions costs* (e.g., those involved in election-related intraparty negotiation, campaigning, and electioneering). For each party, we assume that the relevant opportunity cost is the utility it currently draws from its role in government $s_i + (c_i \times g_i^j)$. $E_i \geq 0$ represents party *i* election-related transactions costs (i.e., the cost of achieving the benefits, b_i, revealed by the event). We do not assume that such transactions costs are the same for all parties. Together, our assumptions about the event and election-related transactions costs imply that party *i* expects a period 2 utility of $b_i - E_i$ from a period 1 election. For reference purposes, we display the game's relevant parameters:

Control of seats	$s_i \in \mathfrak{R}^+$
Initial share of power	$c_i \in [0, 1], c_1 + c_2 = 1, c_o = 0$
Postnegotiation shares of power	$\Sigma_{i \in \{1,2,o\}} c_i^j = 1$
Value of coalition partner *j* to *i*	$g_i^j \in \mathfrak{R}^+$
Postelection prospects	$b_i \in \mathfrak{R}$
Transactions costs	
Election-related	$E_i \in \mathfrak{R}$
Negotiation-related	$K_i \in \mathfrak{R}$

Party *i*'s utility from a period 1 election = $b_i - E_i$.

Utility from current role in government: (also election or negotiation-related opportunity cost) = $s_1 + (c_1 \times g_1^2)$, for the first party, $s_2 + ((1 - c_1) \times g_2^1)$ for the second party, and s_o for the out-party.

Reactions to the Event

After the event occurs, each coalition member has one opportunity to offer a reallocation of power to any other party. An example of such an offer is the following: "If you, party *i*, will coalesce with me, party *j*, then you shall receive share of power c_i^j, and I shall receive share of power $c_j^i = 1 - c_i^j$." For expositional clarity, we denote offered power shares with superscripts representing who power is shared with (e.g., c_i^j). Initial power shares have no superscripts (e.g., c_i).[17]

Recall that we describe the case where any two parties can coalesce and form a government. Thus the consequence of an accepted offer will be either a redistribution of power among members of the initial governing coalition or the formation of a new governing coalition. The first party has the first opportunity to make such an offer.[18] If it does so, then the party to whom the offer is made can either accept or reject it. If it is accepted, then the game ends and the allocation of power that the first party proposed determines period 2 payoffs. If the first party either fails to make an offer or has its offer rejected, then the second party has an opportunity to make an offer. If it is accepted, then the game ends and the allocation of power that the second party proposed determines period 2 payoffs.[19]

If no party is willing to make an offer that another is willing to accept, then there exists the possibility of a vote of confidence. The requirements for such a vote to occur are (1) that no offer is made and accepted and (2) that a parliamentary majority wants such a vote. If the vote is held, and parties controlling a majority of seats vote *no*, then parliament is dissolved, new elections are held, the game ends, and party *i*'s period 2 utility is $b_i - E_i$. Otherwise, the initial governing coalition survives intact, the game ends, and party *i*'s period 2 utility is $s_i + (c_i \times g_i^j)$.

Simplifying Assumptions

Three assumptions allow us to simplify the exposition with only a minimal loss of generality. First, we assume that a failed vote of confidence is necessary and sufficient for dissolution and new elections. To be certain, such a focus simplifies the actual dissolution procedures found in some parliamentary democracies. For instance, some constitutions severely constrain or even prohibit early dissolution (e.g., Norway, Switzerland). Most parliamentary democracies, however, permit early dissolution on terms similar to those assumed here. For simplicity and generality, we abstract away from the discretionary power sometimes vested in the head of state (e.g., as in Germany). Notice, however, that the extension of our findings to many of these cases is straightforward. For instance, in the case where dissolution requires a head of government's or head of state's approval, dissolution is at least marginally more difficult to accomplish. We can represent such difficulty as an election-related transactions cost (by raising E_i). To represent cases where the person vested with the power of dissolution has a partisan bias, we can raise some E_i more than others. To represent the case where early dissolution is prohibited, we can make E_i prohibitively high for all parties. In sum, we assume that in all cases where dissolution is possible, every moment of a coalition's existence rests on the passage of at least an implicit vote of confidence.[20]

Second, we assume that each player has a minimal preference for stability, which manifests itself as a tie-breaking rule for the case where the expected utility from two distinct activities is equal. If all else is equal, then we assume that a party will act to preserve the initial governing coalition. If such an outcome is not possible but all else remains equal, then we assume that a party will act to preserve the current parliament. Simple alterations to this tie-breaking rule affect our findings in a minimal and straightforward fashion.

Third, we assume that making offers may involve a transactions cost. We represent such a cost by assuming that an offering party must pay $K_i \in \Re$. Our motivation for K_i is the party-specific costs of formulating an offer to redistribute power. These costs include the effort required to obtain the approval of party members and constituents.[21] As was true of electoral costs, we assume these negotiation costs may be different for different parties. We also recognize the existence of negotiation-related opportunity costs, such as the policy-making opportunities or rent collection opportunities that parties squander when they engage in coalition bargaining. As we did with election-related costs, we assume that each party's negotiation-related opportunity cost is the utility it currently draws from its role in government $s_i + (c_i \times g_i^j)$.

RESULTS

We now use the model to provide a deductively valid conclusion to the question of primary interest: What are the causes and consequences of coalition termination? Our conclusion is presented as three theorems and a corollary. We prove the first theorem in the Appendix. The validity of the other theorems follows from similar logic (twelve lemmas presented in the Appendix).

We begin by presenting conditions A, B, and C. These conditions are derived from eight lemmas in the Appendix and prove to be critical in the identification of coalition termination dynamics.[22] The Appendix includes a formal statement of each condition.

CONDITION A. *There is a majority that prefers an election to leaving the governing coalition exactly as it was.*

Condition A is satisfied only if either (1) the first and second parties hold a majority of seats and both anticipate higher utility from an election than they do from maintaining the status quo; (2) the first and out-parties hold a majority of seats and both anticipate higher utility from an election than from the continuance of the status quo; or (3) the second and out-parties hold a majority of seats and both anticipate higher utility from an election than from the continuance of the status quo.

CONDITION B. *All offering parties prefer an election to the best acceptable offer they can make.*

Condition B is satisfied if and only if each offering party finds itself in a situation equivalent to the following: the first party anticipates receiving higher utility from an election than it does from either making the offer that both maximizes its own utility and provides higher utility for the second party than an election or making the offer that maximizes its own utility and provides higher utility for the out-party than an election.

CONDITION C. *No offering party prefers the best acceptable offer it can make to leaving the governing coalition exactly as it was.*

Condition C is satisfied if and only if both the first and second parties prefer the status quo to a coalition between the out-party and itself in which the offering party retains nearly all governing power.

Theorem 1 characterizes the necessary and sufficient conditions for a potentially critical event to lead not only to coalition termination but also to dissolution.

THEOREM 1. *The event leads to dissolution if and only if A and B are true.*

Theorem 1 tells us that not every potentially critical event leads to dissolution. For dissolution to occur, there must exist a set of parties that collectively have a parliamentary majority and individually prefer an election to the status quo. Each party that can make an offer must also prefer the anticipated consequences of new elections to the most favorable and acceptable offer it can make.

This result leads us to qualify an important insight offered by Grofman and van Roozendaal, who argue that "anticipation of future electoral gains may cause a certain party or a group of parties to seek to bring down the cabinet at a moment when their anticipated electoral success will be greatest" and hypothesize that "parties terminate cabinets when they expect electoral gains" (1994, 158). Our model suggests that this insight is only partially correct. In coalitional circumstances, a party with favorable electoral prospects will also consider the option of extracting advantages through nonelectoral means (e.g., bargaining with parties that have less favorable prospects). Such renegotiation is particularly likely if some member of the existing coalition has a strong desire to avoid elections. Furthermore, even in noncoalitional two-party systems, where a single governing party can call elections at will, high election costs can suffice to preclude such an action. In sum, *it is simply not true that favorable electoral prospects for any particular party are sufficient to cause a parliament's downfall.*

Having established that a potentially critical event, by itself, does not cause dissolution, we now offer a corollary that describes the conditions under which dissolution is most likely. The validity of corollary 1 follows directly from theorem 1 and the straightforward application of simple comparative statics.

COROLLARY 1. *When it affects whether or not dissolution occurs, an increase in any party's expected postelection utility* (b_i) *or negotiation costs* (K_i) *OR a decrease in any party's election costs* (E_i)*, subjective value of seat control* (s_i)*, or subjective value of coalition membership* (g_i^j) *leads to dissolution. In addition, such changes never prevent dissolution where it otherwise would have occurred.*

Corollary 1 tells us that dissolution is most likely when there exist parties that (1) expect large benefits from an election, (2) face small election costs, (3) face large costs for negotiating nonelectoral transfers of power, (4) derive little value from the seats they currently control, and (5) derive little value from the other coalitions they could enter.

Corollary 1 also suggests a possible interactive effect between time elapsed since the last election and whether a specific event will be critical. To see this effect, first note that most parliamentary democracies possess constitutionally mandated limits on the maximum length of a parliament's term.[23] Before the mandated time is reached, calling new elections is merely an option. However, when this upper bound is reached, new elections must be held.

Now consider this comparative static: if early elections mean that parties sacrifice greater policy-making opportunities and rent collection opportunities than later elections do, and if all else is constant, then as the parliamentary term approaches its upper bound, election-related opportunity costs should decrease. That is, all else constant, g_i^j should be relatively high early in a parliament's term, should decrease continually over that term, and should reach its minimum when parties have no other choice but to hold an election. If this relationship between g_i^j and time holds, then (following corollary 1) an event that does not cause dissolution early in a parliament's term could do so later.[24]

Theorem 1 and corollary 1 imply that *the extent to which a particular event is "critical" is extremely context-dependent*. For instance, if the opportunity costs of having an early election are high, then dissolution requires a large event. Thus if these costs decrease over time, then dissolution requires smaller events as a parliament ages.

Such conclusions run counter to the generally untested assumption of a constant hazard rate (or survival likelihood) that is prevalent in empirical, stochastic models of cabinet stability (Browne, Frendreis, and Gleiber 1986; Cioffi-Revilla 1984).[25] Interestingly, Warwick (1992b) finds impressive cross-national evidence that the hazard rate for executive coalitions increases over time. This is precisely what our model predicts, and we know of no other persuasive behavioral account of this regularity. At a minimum, future attempts to estimate empirically the determinants of cabinet duration should include a component that increases continuously as the constitutionally mandated parliamentary term limit approaches.[26]

We now describe necessary and sufficient conditions for a potentially critical event to have absolutely no effect on the initial governing coalition.

THEOREM 2. *The status quo is maintained if and only if A is false and C is true.*

Theorem 2 first tells us that the current governing coalition's and cabinet's complete survival requires that every parliamentary majority must include at least one pivotal party that prefers the status quo to an election. Theorem 2 also reveals that a coalition's survival requires that each offering party must prefer the status quo to a coalition (with the out-party) in which it retains nearly all governing power. If either requirement is not met, then the current cabinet does not survive—either a nonelectoral reallocation of power between existing coalition members, a coalition-changing nonelectoral reallocation of power, or dissolution and new elections will occur.

Our second theorem implies that coalitional stability is possible in the presence of either favorable election prospects or parties who are able to offer each other greater shares of power than they currently have. Simple comparative statics reveal why this is true: low electoral benefits (low b_i) decrease the threat of an election and a sufficiently unattractive out party (low g_1^o and g_2^o) or high negotiation costs decrease the threat of a nonelectoral reallocation of power. Absent both threats, offering parties have little bargaining power and generally cannot persuade other parties to accept meaningful changes to the status quo. Therefore, we conclude that *stability is fostered by either a dearth of alternate utility-generating opportunities for potential coalition partners or factors that make party-specific negotiation costs or election costs high.*

In all circumstances except those identified in theorems 1 and 2, the event leads to some type of nonelectoral reallocation of power.

THEOREM 3. *The event leads to a nonelectoral redistribution of power if and only if either (1) A is true and B is false or (2) A and C are false.*

While it is beyond our scope here to discuss, in depth, the conditions under which particular types of coalitions will form, we can address several widely held notions about coalition reformation.[27] For example, several recently published formal theories of coalition formation in a three-party legislature predict that the governing coalition will comprise the largest and smallest legislative parties (Austen-Smith and Banks 1988; Baron 1991).[28] Our findings imply that the validity of this prediction is not robust to the introduction of parties who look forward to the next coalition termination or election when they bargain. A further implication is that *the size and bargaining power of political parties need not be positively correlated.*

To see this, notice that a necessary condition for the "largest-smallest" prediction is that the party to whom an acceptable offer is made must control either the smallest or the largest number of seats. Now consider the general case where the largest party, *i*, is in the position to make an offer and there exists a disparity between the value of potential coalition partners, *j* and *x* to it ($g_i^j \neq g_i^x$). All else constant, the offering party should prefer to coalesce with the more valuable coalition partner. If all else is not constant, however, then the offering party must consider the trade-off between the value of a coalition partner (the value of different pies) and the size of the power share that it can retain (the share of each pie that it would receive). If the disparity is sufficiently large (e.g., if one party is a virtual pariah), then the offering party could choose to coalesce with the larger party (it will opt for a smaller piece of a more valuable pie).

In sum, we agree with formal political theorists that if an offer is made, then it will be made to the party whose bargaining power is weakest. However, such behavior does not require the formation of a coalition between the largest and smallest parties. Instead, if an offer is made, then it will be made to the weakest party, where the weakest party is the one that faces

FIGURE 2

The Relationship between Conditions and Outcomes

(A) The vote of confidence will fail

	(B) Election beats best offer	(not B) Best offer beats election
(C) Status quo beats best offer	Dissolution & new elections	Non-electoral reorganization
(not C) Best offer beats status quo	Dissolution & new elections	Non-electoral reorganization

(not A) The vote of confidence will succeed

	(B) Election beats best offer	(not B) Best offer beats election
(C) Status quo beats best offer	Status quo survives	Status quo survives
(not C) Best offer beats status quo	Non-electoral reorganization	Non-electoral reorganization

the most utility-damaging combination of (1) bad electoral prospects, (2) high election costs, (3) high negotiation costs, (4) low-value coalition partner alternatives, and (5) highly valued seats or coalition-related power (that must be forfeited as a result of coalition termination or parliamentary dissolution). Similar distinctions can be drawn between our "weakest party" dynamic, Riker's (1962) prediction concerning minimum winning coalitions, and Axelrod's (1970) prediction of connectedness.[29]

Figure 2 shows the ultimate relationship between our three conditions and the different types of outcomes described in the theorems.

AN EXAMPLE

We now present numerical examples that place our results in a more concrete setting. Some readers have found these examples to be very helpful in understanding which contexts trigger (or fail to trigger) coalition terminations. Others can treat this section as optional reading.

The example's critical assumptions are that the first party controls 45% of the seats ($s_1 = .45$), the second party holds 15% of the seats ($s_2 = .15$), the out-party holds 40% of the seats ($s_o = .40$), the first and second parties have equal shares of power in the initial governing coalition ($c_1 = c_2 = .5$), and all possible coalitions generate the same value for each party ($\forall i, j : g_i^j = 1$). The example's potentially critical event reveals that if election costs are zero, then new elections make the first and out-parties better off—and the second party worse off—than the status quo:

$$b_1 = 1 > .95 = s_1 + c_1 g_1^2$$

$$b_2 = .09 < .65 = s_2 + c_2 g_2^1$$

$$b_o = .91 > .4 = s_o.$$

The variables in our example are the negotiation transactions costs, $K_1 \in \{0, 1\}$, $K_2 \in \{0, 1\}$, and election transactions costs, $\forall i : E_i \in \{0, 1\}$.

In our example, the s_i and c_i add up to one, while the b_i add up to two. What motivates this set of parameters is the example-specific assumption that the source of beliefs about postelection benefits (b_i) are expectations about postelection seat and power shares.

$$\sum_i b_i = 2 = \sum_i s_i + \sum_i c_i g_i^j.$$

In sum, we choose this particular set of parameter values only because they allow us quickly to show how simple variations in electoral or legislative institutions might moderate the effect of a potentially critical event on coalition termination.

Example 1a. Negotiation and election costs are zero.

The outcome of the game is a redistribution of power between members of the initial governing coalition. To see why this is true, notice, in the example's initial assumptions, that the first and out-parties prefer an election to the status quo. Therefore, if no offer is accepted, then new elections are held.

To see why such elections do not take place, we move to the next to last stage of the game, the second party's opportunity to make an offer. The first party requires no less than

$$c_1^2 = \frac{b_1 - E_1 - s_1}{g_1^2} = .55$$

as a prerequisite for coalescence, as such an offer would make it indifferent between acceptance and new elections [$s_1 + (c_1^2 g_1^2) = 1 = b_1 - E_1$]. For similar reasons, the out-party requires no less than

$$c_o^2 = \frac{b_o - E_o - s_o}{g_o^2} = .51.$$

Since making either offer leaves the second party better off than it expects to be after new elections ($b_2 - E_2 = .09$), since all coalitions are equally valuable to the second party (all $g_i^j = 1$) and since the second party benefits by making the smallest offer that another party will accept, the best move for the second party at this stage of the game is to offer $c_o^2 = .51$ to the out-party [$s_2 + ((1 - c_o^2) \times g_2^o) - K_2 = .64 > s_2 + ((1 - c_1^2) \times g_2^1) - K_2 = .6 > .09 = b_2 - E_2$].

To see why the outcome of the game is not a new coalition between the second and out-parties, we move to the previous (and first) stage of the game, the first party's opportunity to make an offer. We now know that if the first party fails to make an acceptable offer, then the second and out-parties will coalesce. As a result, the second and out-parties will not accept any offer that leaves them worse off than will their prospective coalescence. Thus the out-party demands no less than $c_o^1 = .51$ as a prerequisite for coalescence, while the second party demands no less than $c_2^1 = .49$.

If the first party makes no offer, then it will be excluded from the period 2 coalition and will receive utility $s_1 = .45$. If it makes the smallest offer that leaves the out-party at least as well off as a coalition with the second party, then it receives no more than $s_1 + ((1 - (c_o^1 = .51)) \times g_1^o) - K_1 = .94$. Similarly, an acceptable offer to the second party would leave the first party with a payoff of no more than $s_1 + ((1 - (c_2^1 = .49)) \times g_1^2) - K_1 = .96$. Therefore, the first party makes the smallest acceptable offer, $c_2^1 = .49$; the second party accepts; and the game ends. Notice that *example 1a is the only case in which the largest and smallest parties choose to make a new coalition.*

Example 1b. Same as 1a, except that the first party's negotiation cost, K_1, increases from 0 to 1.

The outcome is now a new coalition between the second and out-parties. To see this, notice first that the dynamics of this example are the same as example 1a up to the point where we consider the situation faced by the first party.

Now consider the first party's dilemma. The increase in K_1 makes it prohibitively expensive for the first party to make an acceptable offer to either of the other two parties. In other words, if the first party makes no offer, it will be excluded from the period 2 coalition and will receive utility $s_1 = .45$; if it makes an acceptable offer to the out-party, it will receive no more than $s_1 + ((1 - c_o^1) \times g_1^o) - 1 = -.06$; and if it makes an acceptable offer to the second party, it will receive $s_1 + ((1 - c_2^1) \times g_1^2) - 1 = -.04$. Therefore, the first party makes no offer, the second party offers $c_o^1 = .51$ to the out-party, the out-party accepts, and the game ends. Notice that *example 1b represents the only case in which the minimum winning coalition forms.*

Example 1c. Same as 1b, except that the second party's negotiation cost, K_2, increases from 0 to 1.

The outcome is now dissolution and new elections. To see why this is true, notice first that the dynamics of this example are the same as example 1a up to the point where we consider the second party's opportunity to make an offer. Next notice that the increase in K_2 makes it prohibitively expensive for the second party to make an acceptable offer. Since the first party's negotiation cost is also prohibitively high (see example 1b), neither party makes an offer, the vote of confidence is held, and its failure leads to an election.

Example 1d. Same as 1c, except that each party's election cost increases from 0 to 1.

The outcome is now the status quo. To see this, notice first that the increase in election costs leads each party to prefer the status quo to an election.

$$b_1 - E_1 = 0 < .95 = s_1 + (c_1 \times g_1^2)$$

$$b_2 - E_2 = -.91 < .65 = s_2 + (c_2 \times g_2^1)$$

$$b_o - E_o = -.09 < .4 = s_o$$

This reversal in preferences means that a vote of confidence would pass if held. Therefore, if no offer is accepted, then the status quo is maintained.

Now consider the situation faced by the second party. Because the consequence of no accepted offer is the status quo, neither the first nor the out-parties will accept any offer that provides them with a smaller share of power than they currently have. Thus the first party requires no less than $c_1^2 = .5$ as a prerequisite for coalescence while the out-party will accept an offer that includes any positive share of power. While the second party could have nearly all of the power by coalescing with the out-party, the magnitude of K_2 leads the second party to prefer the status quo to any offer it could make [$s_2 + ((1 - c_o^2) \times g_2^o) - K_2 = .15 - \epsilon < .65 = s_2 + (c_2 \times g_2^1)$], where $\epsilon > 0$ and very small. Therefore, the best move for the second party is to make no offer. Since the situation faced by the first party is nearly identical to that faced by the second, it also makes no offer. Therefore, neither party makes an offer, and the game ends with the initial governing coalition intact.

Example 1e. Same as 1d, except that the first party's negotiation cost, K_1, decreases from 1 to 0.

The result is a new coalition between the first and out-parties. The dynamics of this example are the same as example 1d up to the point where we consider the first party's offer. The out-party will accept any positive share of power as a prerequisite for coalescence while the second party requires no less than its current share of power, $c_2 = .5$. If the first party makes no offer, then the outcome of the game will be the status quo, and it will receive utility $s_1 + (c_1 \times g_1^2) = .95$; if it makes an acceptable offer to the out-party, the most utility it could receive is $s_1 + ((1 - \epsilon) \times g_1^o) - K_1 = 1.45 - \epsilon$; and if it makes an acceptable offer to the second party, the most utility it could receive is $s_1 + ((1 - c_2^1) \times g_1^2) - K_1 = .95$. Therefore, the first party offers the smallest possible share of power to the out-party, the out-party accepts, and the game ends.

CONCLUSION

Our primary conclusion is that external events can deeply affect—but do not wholly determine—the dynamics of coalition bargaining. The effects of external events depend critically on the context in which they take place, the bargaining power of the various parties, and the constraints each party faces. For example, the 1980s breakups of the German and Irish coalitions were not the necessary and automatic outcomes of any environmental shock. On the contrary, they resulted from careful and deliberate adaptation to changing circumstances.[30]

Furthermore, and as McLean anticipated, "One vital factor brought out by none of the coalition theories is that there are two overlapping games going on all the time in parliamentary bargaining. There is coalition-building on the basis of the (known) result of the last election; and bargaining in anticipation of the (unknown) result of the next one. Immediately after an election, the first swamps the second, which gradually increases in relative importance until just before the next election" (1987, 113–14).

Of course, other institutional or organizational features ("structural attributes") may be just as important. The costs that individual parties incur while making or considering coalitional offers are one such factor. The leaders of the Irish Labour party, for example, faced major internal organizational constraints on their ability to negotiate with Fine Gael or any other party. These organizational rules imposed bargaining costs on the Labour party, as well as on any party (e.g., Fine Gael) that attempted to negotiate with it. The precise nature of such institutional constraints is well worth more intensive exploration in future research.[31]

The key implication of our findings is that scholars who want to understand parliamentary decision making need to pay greater attention to the specific nature of critical events. We suggest that information concerning anticipated voter behavior can serve as a gauge of a particular event's critical potential. Though this information need not be critical events' only metric, we believe that previous neglect of electoral incentives may have prevented us from obtaining a more adequate understanding of parliamentary decision making, especially as it regards coalition termination. While politicians cannot always "go to the country," the anticipated voice of the people is never far from the forefront of their concerns. In legislatures vested with dismissal and dissolution powers, expected electoral impact is often what makes a political event "critical" to cabinet stability.

Parliamentary government, the most common democratic regime type, depends on effective delegation of power along a single chain of command. We argue that changes in the control of these governments are not an automatic consequence of environmental shocks. Instead, they are both a reflection of deliberate responses to such events and a reaction to the anticipated feelings of the electorate. The virtues of democracy hinge on precisely such interactions.

APPENDIX

We first present our findings about party behaviors, then we present our findings about the nature of coalition termination. We use the *subgame perfect Nash equilibrium* solution concept to derive our findings. This concept provides deductively valid conclusions and requires that we proceed by backward induction on the extensive form depicted in Figure 1. Thus we begin by characterizing behaviors at the end of the game.

LEMMA 1: The vote of confidence. *The event leads to the dissolution of the parliament and new elections if and only if the first and second parties fail to make an offer that ends the game and there exist parties* i, j, *and* $x \in \{i, j, \emptyset\}$, *such that* $s_i + s_j > .5$ *and* $b_i - E_i > s_i + c_i g_i^x$ *and* $b_j - E_j > s_j + c_j g_j^x$.

In words, Lemma 1 says that if the game advances to its final stage; if there exists a set of parties that together control a majority of seats; and if at least one of these sets contains only parties for whom an election is better than the status quo, then the outcome cannot help but be a lost vote of confidence. Given the assumptions that any majority can force a vote of confidence, and that a lost vote of confidence is a sufficient condition for dissolution and new elections, the validity of Lemma 1 is obvious.

The next-to-last stage of the game involves the reactions to any offer that the second party might make. Lemmas 2 and 3 describe these reactions.

LEMMA 2. *The out-party accepts the second party's offer, call it* c_o^2, *if and only if the first party fails to make an accepted offer and either (1) rejection leads to an election and*

$$c_o^2 \geq \frac{b_o - E_o - s_o}{g_o^2}$$

or (2) rejection leads to the status quo and $c_o^2 > 0$.

LEMMA 3. *The first party accepts the second party's offer, call it* c_1^2, *if and only if the first party fails to make an accepted offer and either (1) rejection leads to an election and*

$$c_1^2 \geq \frac{b_1 - E_1 - s_1}{g_1^2}$$

or(2) rejection leads to the status quo and $c_1^2 > c_1$.

The proof of Lemma 2 reveals the insight as to why these particular reactions take place. The proof of Lemma 3 is omitted because it is similar to the proof of Lemma 2.

Proof of Lemma 2. From the extensive form, it follows that if the out-party accepts the second party's offer, then it must be the case that the first party failed to make an accepted offer. In what follows, we consider only the case where the first party experiences such failure. From Lemma 1 and the common knowledge, the out-party can deduce whether its refusal of the second party's offer will lead to an election or the status quo. If rejection leads to an election, then the out-party receives $s_o + c_o^2 g_o^2$ if it accepts the offer and $b_o - E_o$ if it does not. Thus the out-party should accept the offer if and only if the offer is made and $s_o + c_o^2 g_o^2 \geq b_o - E_o$. Subtracting s_o from each side of the equation and then dividing each side by g_o^2 produces the requirement that

$$c_o^2 \geq \frac{b_o - E_o - s_o}{g_o^2}.$$

Similarly, if rejection leads to the status quo, then the out-party receives period 2 utility $s_o + c_o^2 g_o^2$ if it accepts the offer and s_o (since its share of power in the initial governing coalition is zero) if it does not. This implies that under the threat of the status quo, the out-party should accept if and only if $c_o^2 g_o^2 > 0$. The sufficiency condition follows trivially. Q.E.D.

In sum, *a party's willingness to accept an offer depends on the value of its alternate opportunities: the more valuable the opportunities, the greater the threshold of power the party requires in exchange for foregoing them.* If a party believes that its rejection will lead to an election, then there is no reason for it to accept any offer that provides less utility than will an election. If, by contrast, parties believe that their rejection will lead to the status quo, then they should not accept an offer that provides less utility than the status quo. Since the out-party is not a member of the initial governing coalition, Lemma 2 implies that even a very small share of power would make the out-party better off than it was at the beginning of the game.

As Lemma 4 follows straightforwardly from the utility maximization and common knowledge assumptions, it is offered without proof.

LEMMA 4. *If $K_i > 0$, party i only makes offers that will be accepted.*

It is without a loss of substantive generality that we henceforth simplify the exposition by focusing on the case where $K_i > 0$.

Lemma 5 specifies the conditions under which the second party will make an offer. It also provides detailed information about the party to whom the offer will be made and the size of the offer.

LEMMA 5: The second party's offer

a. *If the second party's failure to make an acceptable offer leads to new elections, if there exists an offer that provides greater period 2 utility than an election to both the first and second parties, and if that offer also provides the second party with greater or equal period 2 utility than would an offer of*

$$\frac{b_o - E_o - s_o}{g_o^2}$$

to the out-party, then the second party offers

$$\frac{b_1 - E_1 - s_1}{g_1^2}$$

to the first party.

b. *If the second party's failure to make an acceptable offer leads to new elections, if there exists an offer that provides greater period 2 utility than an election to both the second and out-parties, and if that offer also provides the second party with greater period 2 utility than would an offer of*

$$\frac{b_1 - E_1 - s_1}{g_1^2}$$

to the first party, then the second party offers

$$\frac{b_o - E_o - s_o}{g_o^2}$$

to the out-party.

c. *If the second party's failure to make an acceptable offer leads to the status quo and if there exists an offer that provides greater period 2 utility to both the second and out-parties than does the status quo, then the second party offers the smallest possible (positive) share of power to the out-party.*

d. *Otherwise, the second party makes no offer, and a vote of confidence ensues.*

Proof of Lemma 5. From Lemmas 1–3 and the common knowledge, the second party can deduce the consequences of its actions. It follows that the second party should make some acceptable offer if and only if either (1) its failure to do so would lead to an election and min $[s_2 + (1 - c_o^2)g_2^o - K_2, s_2 + (1 - c_1^2)g_2^1 - K_2] \geq b_2 - E_2$ or (2) its failure to do so would lead to the status quo and min $[s_2 + (1 - c_o^2)g_2^o - K_2, s_2 + (1 - c_1^2)g_2^1 - K_2] > s_2 + c_2 g_2^1$.

We first consider the second party's strategy in the case where its failure leads to an election. In this case, the smallest offer that the first party will accept is

$$c_1^2 = \frac{b_1 - E_1 - s_1}{g_1^2},$$

while the smallest offer that the out-party will accept is

$$c_o^2 = \frac{b_o - E_o - s_o}{g_o^2}.$$

Therefore, if

$$s_2 + \left(1 - \frac{b_1 - E_1 - s_1}{g_1^2}\right) g_2^1 - K_2 \geq \max\left[s_2 + \left(1 - \frac{b_o - E_o - s_o}{g_o^2}\right) g_2^o - K_2, b_2 - E_2\right],$$

then the second party offers

$$\frac{b_1 - E_1 - s_1}{g_1^2}$$

to the first party. Similarly, if

$$s_2 + \left(1 - \frac{b_o - E_o - s_o}{g_o^2}\right) g_2^o - K_2 > s_2 + \left(1 - \frac{b_1 - E_1 - s_1}{g_1^2}\right) g_2^1 - K_2$$

and if

$$s_2 + \left(1 - \frac{b_o - E_o - s_o}{g_o^2}\right) g_2^o - K_2 \geq b_2 - E_2,$$

then the second party offers

$$\frac{b_o - E_o - s_o}{g_o^2}$$

to the out-party. Otherwise, the second party makes no offer.

We now consider the case where the second party's failure to make an acceptable offer leads to the status quo. In this case, the smallest offer that the first party will accept is $c_1^2 = c_1 + \varepsilon$, where $\varepsilon > 0$ and small, and the smallest offer that the out-party will accept is $c_o^2 = \varepsilon$. Since making such an offer to the first party necessarily provides the second party with less utility than making no offer, making any offer to the first party under these circumstances is a dominated strategy. Thus, if $(1 - \varepsilon)g_2^o - K_2 > c_2 g_2^1$, then the second party offers ε to the out-party. Otherwise, the second party makes no offer. Q.E.D.

In short, if the second party wants its offer to be accepted, then it must offer a share of power that is at least as large as the amount specified in Lemmas 2 or 3. Notice, however, that the more power the second party offers to another party, the less power it has for itself. Thus, if there exists a set of offers that can make both the second party and one other party better off than whatever the consequence of rejecting the offer would be (itself determined by the anticipated result of the vote of confidence), then the second party is best off making the smallest offer that another party will accept. If the second party's negotiation costs are sufficiently high or if both the first and the out-party are in a position to demand a large share of power, then the second party passes on its opportunity to make an offer.

The dynamics underlying the first party's opportunity to make an offer and the other parties' responses to such an offer, are very similar to the dynamics just described. For instance, consider Lemmas 6 and 7, which are offered without proof because of their similarity to Lemmas 2 and 3.

LEMMA 6. *The out-party accepts the first party's offer, call it c_o^1, if and only if (1) rejection leads to an election and*

$$c_o^1 \geq \frac{b_o - E_o - s_o}{g_o^1},$$

(2) rejection leads to the status quo and $c_o^1 g_o^1 > 0$, *(3) rejection leads to its exclusion from the period 2 coalition and* $c_o^1 g_o^1 \geq 0$, *(4) rejection leads to a coalition with the second party made under the threat of an election and*

$$c_o^1 \geq \frac{b_o - E_o - s_o}{g_o^1},$$

or (5) rejection leads to a coalition with the second party made under the threat of the status quo and $c_o^1 g_o^1 \geq \varepsilon$.

LEMMA 7. *The second party accepts the first party's offer, call it* c_2^1, *if and only if (1) rejection leads to an election and*

$$c_2^1 \geq \frac{b_2 - E_2 - s_2}{g_2^1},$$

(2) rejection leads to the status quo and $c_2^1 > c_2$, *(3) rejection leads to a coalition with the first party made under the threat of an election and*

$$c_2^1 \geq 1 - \frac{b_1 - E_1 - s_1}{g_1^2} - \frac{K_2}{g_2^1},$$

(4) rejection leads to a coalition with the out-party made under the threat of an election and

$$c_2^1 \geq \frac{\left(1 - \dfrac{b_o - E_o - s_o}{g_o^2}\right) g_2^o - K_2}{g_2^1},$$

(5) rejection leads to a coalition with the out-party made under the threat of the status quo and

$$c_2^1 \geq \frac{(1 - \varepsilon) g_2^o - K_2}{g_2^1}.$$

In short, the minimum share of power that a party requires for coalescence depends on the value of its other opportunities. For example, if the consequence of the first party's failure is an election, then the minimum share of power that party $i \in \{2, o\}$ is willing to accept is

$$c_i^1 \geq \frac{b_i - E_i - s_i}{g_i^1},$$

as before. Notice that this minimum acceptable share is increasing in the receiving party's electoral prospects, decreasing in its election costs, and decreasing in the value it derives from its relationship to the initial governing coalition. Similarly, if the consequence of the first party's failure is the status quo, then the second party is willing to accept no less than its current share of power, while the out-party (who has no current share of power) is willing to accept any positive share. If the consequence of the first party's failure is that the second party will make an offer of the type described in Lemma 5, then no party will accept a share of power that provides it with less utility than the second party's offer would.

As was true of the second party, the first party chooses a coalition partner based on its value (g_1^i) and on how much power it requires for coalescence. Lemmas 8–12 describe the first party's decision at the game's first decision node and the outcome of the game. What distinguishes each of these lemmas from the others in this set is the consequence of the first party's failure to make an acceptable offer. Notice that across Lemmas 8–12 the necessary conditions are collectively exhaustive. Notice also that within each lemma, the outcomes are collectively exhaustive. Since the collective proof of these lemmas follows the same general logic as the proof of Lemma 5, we contend that the proof of each should be obvious. Notice further that when there exist multiple offers that make the first party—and the party to whom the offer is made—better off than the consequence of rejection, the smallest of these offers is made.

LEMMA 8. *If the consequence of the first party's failure to make an acceptable offer is an election, then*

Additional Contingency	First's Offer	Result
$\left(1 - \frac{b_o - E_o - s_o}{g_o^1}\right) g_1^o > \left(1 - \frac{b_2 - E_2 - s_2}{g_2^1}\right) g_1^2$ and		
$\left(1 - \frac{b_o - E_o - s_o}{g_o^1}\right) g_1^o \geq b_1 + K_1 - E_1 - s_1$	$\frac{b_o - E_o - s_o}{g_o^1}$	first and out coalesce
$\left(1 - \frac{b_2 - E_2 - s_2}{g_2^1}\right) g_1^2 \geq \max\left[\left(1 - \frac{b_o - E_o - s_o}{g_o^1}\right) g_1^o, b_1 + K_1 - E_1 - s_1\right]$	$\frac{b_2 - E_2 - s_2}{g_2^1}$	first and second redistribute
$b_1 + K_1 - E_1 - s_1 > \max\left[\left(1 - \frac{b_2 - E_2 - s_2}{g_2^1}\right) g_1^2, \left(1 - \frac{b_o - E_o - s_o}{g_o^1}\right) g_1^o\right]$	none	dissolution and new election

LEMMA 9. *If the consequence of the first party's failure to make an acceptable offer is a coalition between the first and second parties made under the threat of an election, then*

Additional Contingency	First's Offer	Result
$b_1 - E_1 - s_1 + \frac{g_1^2 K_2}{g_2^1} - K_1 > b_1 - E_1 - s_1$ and		
$b_1 - E_1 - s_1 + \frac{g_1^2 K_2}{g_2^1} - K_1 \geq g_1^o - K_1$	$1 - \frac{b_1 - E_1 - s_1}{g_1^2} - \frac{K_2}{g_2^1}$	first and second redistribute
$g_1^o - K_1 > \max\left[b_1 - E_1 - s_1 + \frac{g_1^2 K_2}{g_2^1} - K_1, b_1 - E_1 - s_1\right]$	0	first and out coalesce
$b_1 - E_1 - s_1 \geq \max\left[b_1 - E_1 - s_1 + \frac{g_1^2 K_2}{g_2^1} - K_1, g_1^o - K_1\right]$	none	first and second redistribute

LEMMA 10. *If the consequence of the first party's failure to make an acceptable offer is a coalition between the second and out-parties made under the threat of an election, then*

Additional Contingency	First's Offer	Result
$\left(1-\frac{b_o-E_o-s_o}{g_o^1}\right)g_1^o > \left(1-\frac{\left(1-\frac{b_o-E_o-s_o}{g_o^2}\right)g_2^o-K_2}{g_2^1}\right)g_1^2$ and $\left(1-\frac{b_o-E_o-s_o}{g_o^1}\right)g_1^o \ge K_1$	$\frac{b_o-E_o-s_o}{g_o^1}$	first and out coalesce
$\left(1-\frac{\left(1-\frac{b_o-E_o-s_o}{g_o^2}\right)g_2^o-K_2}{g_2^1}\right)g_1^2 \ge \max\left[\left(1-\frac{b_o-E_o-s_o}{g_o^1}\right)g_1^o, K_1\right]$	$\frac{\left(1-\frac{b_o-E_o-s_o}{g_o^2}\right)g_2^o-K_2}{g_2^1}$	first and second redistribute
$K_1 > \max\left[\left(1-\frac{\left(1-\frac{b_o-E_o-s_o}{g_o^2}\right)g_2^o-K_2}{g_2^1}\right)g_1^2, \left(1-\frac{b_o-E_o-s_o}{g_o^1}\right)g_1^o\right]$	none	second and out coalesce

LEMMA 11. *If the consequence of the first party's failure to make an acceptable offer is a coalition between the second and out-parties made under the threat of the status quo, then*

Additional Contingency	First's Offer	Result
$(1-\varepsilon)g_1^o > \left(1-\frac{(1-\varepsilon)g_1^o-K_2}{g_2^1}\right)g_1^2$ and $(1-\varepsilon)g_1^o \ge K_1$	ε	first and out coalesce
$\left(1-\frac{(1-\varepsilon)g_2^o-K_2}{g_2^1}\right)g_1^2 \ge \max[(1-\varepsilon)g_1^o, K_1]$	$\frac{(1-\varepsilon)g_2^o-K_2}{g_2^1}$	first and second redistribute
$K_1 > \max\left[\left(1-\frac{(1-\varepsilon)g_2^o-K_2}{g_2^1}\right)g_1^2, (1-\varepsilon)g_1^o\right]$	none	second and out coalesce

LEMMA 12. *If the consequence of the first party's failure to make an acceptable offer is the status quo, then*

Additional Contingency	First's Offer	Result
$(1-\varepsilon)g_1^o - K_1 > c_1 g_1^2$	ε	first and out coalesce
$c_1 g_1^2 \ge (1-\varepsilon)g_1^o - K_1$	none	status quo

Formal Statements of Conditions A, B, and C

CONDITION A. *There exists a majority that prefers an election to the status quo.*

$$(s_1 + s_2 > .5 \text{ and } b_1 - E_1 > s_1 + c_1 g_1^2 \text{ and } b_2 - E_2 > s_2 + c_2 g_2^1)$$

or

$$(s_1 + s_o > .5 \text{ and } b_1 - E_1 > s_1 + c_1 g_1^2 \text{ and } b_o - E_o > s_o)$$

or

$$(s_2 + s_o > .5 \text{ and } b_2 - E_2 > s_2 + c_2 g_2^1 \text{ and } b_o - E_o > s_o).$$

CONDITION B. *Offering parties prefer an election to the best acceptable offer they can make.*

$$b_i - E_i > \max\left[s_i + \left(1-\frac{b_j-E_j-s_j}{g_j^2}\right)g_i^1 - K_i,\ s_i + \left(1-\frac{b_o-E_o-s_o}{g_o^2}\right)g_i^o - K_i\right],$$

where

$$\frac{b_i-E_i-s_i}{g_i^j}$$

is the minimum offer that party *i* will accept from party *j* under the threat of an election.

CONDITION C. *No offering party prefers the best acceptable offer it can make to the status quo.*

$$(1-\varepsilon)g_2^o - K_2 \le c_2 g_2^1 \text{ and } (1-\varepsilon)g_1^o - K_1 \le c_1 g_1^2,$$

where $\varepsilon > 0$ is very small.

Proof of Theorem 1. Notice that the only conditions under which the event can lead to dissolution are specified in Lemma 8. Conditions A and B specify the full set of such conditions.

Now suppose that dissolution does not necessitate A. Since at least one set of two parties must have enough seats to constitute a majority, not A implies that neither

$$(b_1 - E_1 > s_1 + c_1 g_1^2 \text{ and } b_2 - E_2 > s_2 + c_2 g_2^1)$$

nor

$$(b_1 - E_1 > s_1 + c_1 g_1^2 \text{ and } b_o - E_o > s_o)$$

nor

$$(b_2 - E_2 > s_2 + c_2 g_2^1 \text{ and } b_o - E_o > s_o).$$

Thus, if not A, then there exists no majority that prefers an election to the status quo. Since a majority is required to defeat the vote of confidence and since a defeated vote of confidence is required for dissolution, dissolution necessitates A.

Now, suppose that dissolution does not necessitate B. Not B implies that at least one of the following statements is not true:

$$b_2 - E_2 > \max\left[s_2 + \left(1 - \frac{b_1 - E_1 - s_1}{g_1^2}\right)g_2^1 - K_2,\ s_2 + \left(1 - \frac{b_o - E_o - s_o}{g_o^2}\right)g_2^o - K_2\right]$$

and

$$b_1 - E_1 > \max\left[s_1 + \left(1 - \frac{b_2 - E_2 - s_2}{g_2^1}\right)g_1^2 - K_1,\ s_1 + \left(1 - \frac{b_o - E_o - s_o}{g_o^1}\right)g_1^o - K_1\right].$$

If one of the two statements are not true, then at least one of the offering parties can make an offer that will be accepted and should do so. If at least one party makes such an offer, however, then the vote of confidence will not be held. Since a vote of confidence is required for dissolution, dissolution necessitates B. Sufficiency follows trivially. Q.E.D.

Notes

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1. The *Irish Times* carried the following reflection on the 1987 fall of the Fine Gael–Labour coalition: "It was the best of Governments; it was the worst of Governments" (as quoted in Girvin 1987, 9).

2. As part of a "constitutional crusade," Prime Minister FitzGerald pressed a referendum to legalize divorce under quite restrictive circumstances. FitzGerald's design ran into considerable ecclesiastical and popular resistance, however, and in June 1986 his initiative was voted down by a humiliating 63% of the voters (Farrell and Farrell 1987, 234).

3. Fianna Fail finally abandoned this policy in 1989, when, after much soul searching, it joined a coalition government with the Progressive Democrats (Laver 1992).

4. King and his colleagues (1990), Alt and King (1994), Warwick (1992 a and b), and Warwick and Easton (1992) all seek in various ways to unify the critical events and structural attributes perspectives. None of them, however, have developed a micropolitical theory.

5. For instance, Cioffi-Revilla maintains that "Italian governments are subject to random political shocks and crises, having numerous manifestations such as scandals, parliamentary defeats, pivotal defections, and other occurrences" (1984, 334).

6. Recent exceptions to this rule include Austen-Smith and Banks 1988; Grofman and van Roozendaal 1994; and Strøm 1990.

7. For a thorough defense of the unitary-actor assumption in static models of parliamentary bargaining, see Laver and Schofield 1990, chap. 2.

8. We expand on each of these assumptions in turn. As is true of other formal models of parliamentary interaction (Austen-Smith and Banks 1988; Baron 1991), we can use our three-party model to explain the dynamics of an *n*-party bargaining situation by assuming that at least one of our parties is actually a protocoalition. This expansion is problematic only to the extent that the unitary-actor assumption does not carry over well to the case of the protocoalition. With regard to our knowledge assumption, we assume that (1) all players know the rules of the game and that (2) every player knows what every other player believes about every aspect of the game, every player knows that every player knows this, and so on. This particular assumption (as opposed to the assumption that all parties have full information about every aspect of the game) makes our results robust to the class of assumptions where all players, while possibly uncertain about the actual values of game's parameters, have a basis for sharing common beliefs about them.

9. Our model shows how parties react to a particular circumstance. The conditions under which our claims hold are sufficiently robust so that we can describe reactions to a relatively broad set of circumstances. Like most game-theoretic models, ours is a static model; it focuses on a particular moment in time. However, its play does not prevent multiple plays of the same game in the future, nor does it prevent players in any particular play of the game from making present decisions based on concrete, subjective beliefs about different possible futures. Therefore, we can (and do) use comparative statics to draw dynamic (nonstatic) conclusions about the causes and consequences of coalition termination.

10. For expositional simplicity, we do not describe the case where grand coalitions are possible. However, we need not alter the model to describe such a case. For example, we can easily simulate grand coalition formation by conceptualizing either the first or the second party as a previously formed coalition of more than one party (or protocoalition) and by setting the share of seats controlled by the out-party sufficiently close to zero. Similarly, the formation and resilience of single-party or minority governments can also be understood using special cases of our model.

11. We make no assumptions about the relative value of different appointments or about whether cabinet and other portfolios are valued for policy or patronage reasons. Therefore, we use the term *share of power* as opposed to the term *number of portfolios* or *number of noncabinet appointments* in recognition of the possibility that some appointments are more valuable than others. For instance, prime ministers and finance ministers typically have broader policy responsibilities and greater agenda powers than other cabinet ministers. For more on this topic, see Breton 1991 and Laver and Shepsle 1993, 1994.

12. Stated another way, we enclose the usual spatial model of parliamentary bargaining into a "black box" by assuming that the parties share common beliefs about the value of the gains from trade that can be created by each possible coalescence.

13. Since our conception of coalition-specific benefits is also party-specific, we are not limited to the case where coalition negotiations are purely zero-sum situations in which parties compete over nondivisible private goods. Put another way, *either, neither, or both* members of a coalition can derive high utility from membership.

14. Since Downs (1957), the theoretical literature on voter and party behavior has been heavily committed to the assumption that parties are vote-maximizers or at least vote-seekers. Other literature on political parties variously portrays them as seekers of power, office, or policy outcomes (Strøm 1990). Our construction allows us to accept each of these motivations as plausible components of a party objective function.

15. Recall that we describe the case where $s_i \in [0, 1]$, $\Sigma_{i\in\{1,2,o\}}\, s_i = 1$. Notice also that we do not impose similar restrictions on the value of g_i^j. Together, these assumptions imply that our results are robust to a wide array of assumptions about the extent to which parties favor seats or governing power. To represent the case where party i's main interest is in the retention of seats, we set g_i^j low. To represent the case where the same party is primarily interested in governing, we set g_i^j high. Since all elements of the utility functions are party-specific, our results are also robust to cases where parties differ in the weight attached to seats or coalition membership in their objective functions.

16. For example, suppose that the first party cares only

about its ability to form a single-party majority government in period 2, while the second party cares only about the number of period 2 seats it will control. We would then characterize the value of b_1 as increasing only in the first party's belief about the subjective probability that it will be able to form a single-party majority government in period 2, given a period 1 election. Similarly, we would characterize the value of b_2 as increasing only in the second party's expectation of the number of seats that it will control in period 2 given a period 1 election. As a result, the value of b_i in our model is affected by common beliefs about the likelihood and severity of subsequent postelection shocks that would cause the game we describe to be played again in the future.

17. In equilibrium, parties can make and accept offers of power shares that are less than zero and greater than one. Allowing such a range of offers simplifies the technical statement of the model considerably. Furthermore, since we do not allow parties to opt out of the game, the actual numerical value of the offer is irrelevant. All that matters is the value of the offer relative to the value of a party's other opportunities. Substantively, we can think of offers that are less than zero or greater than one as examples of rent-seeking behavior by powerful parties who not only have the power to keep all of the valuable offices and policymaking opportunities for themselves but also can extract value from other parties in exchange for a delay in an event (i.e., an election) that will make them even worse off (than they are under the terms of an unfavorable agreement.) In sum, the added notational complexity of restricting c_i^j to [0, 1] would be worthwhile only if the focus of our research was "who gets what." Similar concerns lead us to describe the case where the out-party does not make offers. To see that only minimum generality is lost by this assumption, it is sufficient to notice that all possible two-party coalitions (first-second, first-out, second-out) can be outcomes in our model. Notice also that the conceptual basis of the three key conditions does not depend on whether or not the out-party can make offers. In sum, the added notational complexity of allowing the out-party to make offers (the number of endnodes in the game would increase by 54) would be worthwhile only if the focus of our research was "who gets what." It is not.

18. Since the first party's *only* defining characteristic is that it has the first opportunity to make an offer, this assumption is innocuous.

19. Why do we limit coalition members to one offer each? Our intent is to identify conditions under which parties will put an end to an existing governing coalition. For a particular party to want to make a sequence of offers, it must expect that an early offer in the sequence will be rejected and that a later one will be accepted. For this sequence of events to occur, the value of some parameter *must* change between the time that the first and second offers are made. However, our exogenous determination assumption implies that no important parameter values change during the game, and the common knowledge assumption implies that there is nothing that parties can learn about each other's preferences during the game. Therefore, within our game, there would be no reason to make multiple offers. Of course, in reality, multiple offers are possible, but for an early offer in a sequence to be rejected and a later one accepted, either some important parameter value must change (e.g., the present value of coalescence) or learning must take place. Since ours, like most game-theoretic work, is a static model, the consequence of each possibility can only be viewed with comparative statics based on multiple, independent plays of our game.

20. For a more comprehensive cross-national comparison of the variation in dissolution powers, see Laver and Schofield 1990, 64.

21. To the extent that such costs are coalition-specific, rather than party-specific, they can be represented by decreasing the value of the relevant g_i^j terms.

22. Condition A is derived from Lemma 1. Condition B is derived from Lemmas 2–3 and 5–8. Condition C is derived from Lemmas 2–3, 5–7, and 11.

23. King and his colleagues (1990) refer to this as the "constitutional interelection period."

24. Parties must forfeit currently held seats and coalition membership when an election is held. To the extent that a party derives value from the seats they currently hold or their membership in the current governing coalition, the fact that they are lost at election time means that all else constant, all s_i and g_i^j should decay as a parliament ages—converging to zero when there is no choice but to hold an election. If the values decrease as the age of a parliament increases and all else is constant, then (again, following Corollary 1) an event that does not lead to dissolution early in a parliament's term could do so later.

25. This functional form expressed in the Poisson model has been questioned on theoretical grounds by Warwick and Easton (1992), and empirical studies have provided little support. Kaashoek (1993) finds that the duration distribution varies substantially across political systems, in ways that are generally not consistent with the exponential distribution implied by the Poisson model.

26. One important step in this direction is offered by King and his colleagues (1990), who use the fact that a disproportionate number of cabinets are terminated in the final year of a parliament's maximum allowable term to estimate temporal and structural components of cabinet stability. Warwick and Easton (1992), Alt and King (1994), and Diermeier and Stevenson (1994) build upon the base of King and his colleagues (1990) and offer thorough discussions of the application of different hazard rate functions to the analysis of cabinet stability.

27. The technical details of "who coalesces with whom" (which are tangential to the thrust of the argument in the text) and the terms under which coalescence takes place are completely described in Lemmas 5 and 8–12.

28. Our intent here is not to cast general aspersion on these particular models, which are novel and compelling in many respects and are among the best work in the field. Our disagreement with these models is limited to the stated prediction.

29. For instance (as the argument just given suggests), the weakest party need not control the smallest number of seats. Therefore, the minimum winning coalition need not form. With respect to connectedness, our results suggest that coalitions need not contain the parties whose policy preferences are most closely related. To see this, consider that to the extent that policy preferences play a role in our analysis, we believe that they are represented in the g_i^j terms. That is, we argue that a coalition containing parties with complementary policy agendas is likely to generate greater utility for its members (high g_i^j and g_j^i) than would a coalition containing parties with conflicting policy agendas. However, even if this relationship between policy and g_i^j terms holds, it does not necessarily follow that the most policy-friendly coalition will form. For instance, the first party may be better off making an offer to a party for whom g_i^j is small than it would be by making an offer to a party for whom g_i^j is large, when the latter party requires a much larger share of power as a prerequisite for coalescence.

30. The difference in outcomes between the two cases is at least consistent with our conclusions. The Irish Labour party was ultimately willing to let the coalition fail a confidence vote (condition A held), and given Fianna Fail's reluctance to coalesce, there was no other offer that could beat an election (condition B held). Theorem 1 leads us to expect dissolution in such a circumstance. For the FDP, however, an immediate election was the worst possible outcome (condition B did not hold). Given the Social Democrats' willingness to let the coalition be voted down (condition A held), Theorem 3 leads us to expect a nonelectoral redistribution of power.

31. Political scientists have only just begun to explore effects of institutional arrangements such as the constructive vote of no confidence, constraints on bargaining sequences, and specific rules concerning parliamentary dissolution (see, e.g., Bawn 1993; Laver and Schofield 1990; and Strøm, Budge, and Laver 1994).

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RESEARCH NOTES

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WALTZ, DURKHEIM, AND INTERNATIONAL RELATIONS: THE INTERNATIONAL SYSTEM AS AN ABNORMAL FORM

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Drawing on Emile Durkheim's Division of Labor in Society, *I offer a typology of international systems. Previous uses of Durkheim to describe international systems suffer a number of conceptual errors and therefore are at variance with the spirit and intention of Durkheim's work. A deeper reading of Durkheim usefully draws attention to the moral basis for society and thus the problems with defining international systems solely in terms of power distributions. Further, rereading Durkheim offers a much richer typology than the simple distinction between mechanical and organized societies, affording in turn fresh insights into change in the international system. The* abnormal forms *of the division of labor offer the best description of the contemporary international system.*

In contemporary debates, the main criticisms leveled against Kenneth Waltz's (1979) *Theory of International Politics* include its difficulties in defining such central concepts as power and balance of power, inattention to important internal determinants of foreign policy, and an inadequate account of change in international relations. Of these, the most important is Waltz's controversial and problematic view of change in world politics. Waltz's theory is highly ahistorical, leading him to find striking continuity in the basic nature of international politics. He writes: "Over the centuries, states have changed in many ways, but the quality of international life has remained much the same" (p. 111). Other theorists disagree with Waltz's formulation of the problem of social change. John Gerard Ruggie observes:

> Waltz's views have policy implications that cause displeasure and even distress in a variety of intellectual constituencies most directly concerned with those policy issues: other realists hotly dispute Waltz's benign assessment of recent changes in the correlation of forces between the United States and the Soviet Union; liberals, his dismissal of the global integration of economic processes; Third World supporters, his stress on the virtues of inequality; and world-order advocates, his general vision of the nature of the international system and the range of possibilities it offers. (1986, 141)

To find, for example, the social and political relations of the principalities and fiefdoms of feudal Europe to be qualitatively equivalent to those of the contemporary state system strikes Ruggie as profoundly mistaken. Moreover, many would argue that, rather than continuity, the mounting problems facing the world call for global solutions requiring extensive global cooperation, a significant break with past practices. Some assert further that the scope and magnitude of nuclear proliferation, conventional weapons proliferation, environmental degradation, poverty, hunger, and more might require fundamental political reorganization—perhaps even the formation of a world government. Yet a basic theme of Waltz's influential theory is that any political response that requires institutional transformation or close cooperation among competing states is unlikely and probably impossible to achieve. Rather, the anarchic structure of international politics promises to perpetuate the self-regarding pursuit of power by sovereign nation-states. If ideas affect political action, then the view that Waltz forwards is distinctly disempowering to those who urge institutional change to deal with worsening global problems. Thus, both for conceptual and for policy reasons, the theoretical status of change lies at the heart of debates over international politics.

I shall challenge Waltz's theory by questioning his use of Emile Durkheim's ([1893] 1964) classic, *Division of Labor in Society*, to lend authority to the conclusion that the struggle for power among similar, self-regarding political units—states—is here to stay. On the contrary, a careful reading of Durkheim shows that the theoretical framework that he advances supports Waltz's opponents in the debate over change and transformation more than it does Waltz's own static theoretical model. To anticipate, Waltz argues that Durkheim's concept of "mechanical solidary society" offers a model of unchanging social relationships parallel to his own theory of international politics. Durkheim's "social segments" become Waltz's sovereign states, or "like units", and the segmentation of anarchic international society becomes, likewise, an insurmountable barrier to international integration or interdependence and thus to global transformation (Waltz 1986, 326). Yet Waltz's interpretation of Durkheim ignores that the main subject of *The Division of Labor in Society* is progress or, if not progress, then social and moral change. Indeed, Durkheim's analysis indicates that anarchy itself may change and thus that the nature of international politics will not and has not displayed the "striking sameness" that Waltz observes. Further, if one is to remain faithful to Durkheim's meaning, one cannot understand the transformation of international social relationships solely in terms of shifts in the distribution of capabilities across autonomous, acting "like units." Rather, to understand change in and of the international system requires attention to important variables that press societies toward new forms of social relationships: population growth, the advance of the division of labor, the effects of competition, the development of improved technology of communication and transportation, and the response to such variables in the moral basis of society.[1]

I shall review the debate in international theory, beginning with a summary of Waltz's theory of international politics, and then outline the main features of *Division of Labor in Society* relevant here. With these tasks completed, I turn to Waltz's use of Durkheim in *Theory of International Politics* and elsewhere, after which I explicate criticisms forwarded by John G. Ruggie and Richard K. Ashley and Waltz's reply. An assessment of this debate shows that Waltz's critics have failed to identify the errors that Waltz commits in classifying political systems according to Durkheim's framework. Yet I also argue that although Waltz misuses Durkheim's ideas, the suggestion to employ them in international relations theory is a good one. After a critique of Waltz's use of Durkheim's *Division of Labor in Society,* I offer a typology of international systems employing Durkheim's social categories and postulate that the contemporary international system is best classified as an *abnormal form* of the division of labor.

DURKHEIM AND THE DEBATE IN INTERNATIONAL THEORY

Waltz's *Theory of International Politics*

Waltz posits that political systems comprise a set of interacting units and the structural characteristics that make it possible to think of these interacting units as a distinct whole. States, he assumes, are the defining units of an international system. He identifies three essential structural variables pertaining to all political systems, both international and domestic: their *ordering principle,* the *differentiation of function* among the units, and the *distribution of capability* across the units. To clarify his point, Waltz compares the differences between international systems and domestic society in terms of these three variables.

In regard to ordering principle, domestic society is hierarchical; that is, it displays lines of authority and relations of super- and subordination. Domestic society comprises differentiated units—in other words, a division of labor. Units in domestic society depend upon one another, and they can do so because the authority that structures social relations hierarchically ensures the units' security and survival. Finally, although a strictly hierarchical system would negate the distribution of capability as a variable, because the imposition of authority is never complete, domestic society's units do show a distribution of capability. Hence, this distribution can to some degree help us to predict outcomes for domestic society, although less so than for international political systems.

International systems display the ordering principle of anarchy, not hierarchy. Units exist in relations of coordination rather than super- and subordination. Waltz infers from this that the units must duplicate one another's functions. Because international competition threatens the units' existence, they dare not differentiate or develop a division of labor and thereby become dependent upon one another. The absence of security resulting from anarchy forces them to see to their own welfare and provide for their own needs. From this absence of differentiation, it follows that differentiation "drops out" of the picture. Units differ not in what they do but in how effectively they perform like functions. Power, then, is the only significant systemic, or structural, variable.

From these propositions, Waltz arrives at a "positional picture" of the international system. This picture, he emphasizes, is highly abstract, including nothing at the "structural level" except what is necessary to predict broad outcomes. As long as the system remains anarchic, the main thing that one needs to know is the distribution of capabilities—the polarity structure—to project the likely behavior of the system. Regarding behavior, Waltz concludes that bipolar systems are most stable, and that inequalities in the distribution of power make a limited development of the international division of labor possible.

Durkheim's *Division of Labor in Society*

Waltz's purpose in writing *Theory of International Politics* was to develop a deductive, systemic model of amoral power politics in the international system that would help to explain and predict system behavior. By contrast, Durkheim's purpose in writing *The Division of Labor in Society* was to assess the moral significance of the increasing specialization of function that marks modern society. Although Waltz solicits Durkheim's aid in his project, eventually, these two different purposes come into conflict, forcing Waltz to ignore important elements of Durkheim's theory.

Durkheim attempts to explain and reconcile two contradictory consequences of the increasing specialization of society. On the one hand, both the increases in society's productive power and the individual's capacity fully to develop his or her talents and skills seem to depend upon a deepening division of labor. On the other hand, this specialization seems to exacerbate competitive instincts, emphasize the differences between people, and destroy traditional social relationships that provide social cohesion. The church, guild, extended family, and social hierarchies had, with the development of capitalism, come under pressure as individualistic pursuit of self-interest had replaced the older system of feudal obligations. Hence, industrial society seemed caught in an acute dilemma between progress and social solidarity.

Durkheim asked whether a way out of this dilemma could be found which would promote individual freedom, preserve social solidarity, and avoid revolutionary upheaval. To begin, he presumes that the advance of the division of labor could not be reversed. Its advance depends upon factors beyond manipulation: it is, ultimately, the "derived fact" that increasing *dynamic density* and *social volume* produce (1964, 260). *Dynamic density* refers to the circumstance in which "there are more individuals sufficiently in contact to be able to act and react upon one another" (p. 257). An increase in dynamic density comes about because of concentration of population, urbanization, and improvements in the means of communication and transport. These factors place competitive pressure on individuals and social units, and this pressure leads them to differentiate themselves. Further, increasing social volume, or the increase in the numbers of individuals, adds to the impetus toward differentiation.

Dynamic density and social volume, as the causal bases for an increasing level of competition, thus ultimately result in the "effacement of the social segments" of mechanical society and a greater division of labor. In fact, Durkheim considered this "effacement" under the pressures of (mostly) dynamic density a virtual inevitability (ibid., 380). Significantly, only the effacement of the segments makes way for the full development of the division of labor; otherwise, the social segments present an insurmountable obstacle to its advance. Durkheim concludes: "We can then formulate the following proposition: *The division of labor varies in direct ratio with the volume and density of societies, and if it progresses in a continuous manner in the course of social development, it is because societies become regularly denser and generally more voluminous*" (p. 262, emphasis original).

Durkheim argues further that contrary to the proposition that the process just described would lead inevitably to the loss of social solidarity, the division of labor could provide an outlet for the intense pressures of competition that increasing dynamic density and social volume produce. The division of labor cannot put an end to competition, but it can provide society with a "mellow denouement" of the struggle for existence in mutually beneficial specialization. Rather than the division of labor destroying social solidarity through excessive individuation and specialization, modern society could develop its own form of social solidarity—*organic* solidarity—as a *consequence* of the advance of the division of labor. Simultaneously, specialization—far from restricting individual freedom—might free the individual from oppressive traditional moral structures insofar as they weaken and eventually dissolve with the advance of organic solidarity. Thus social solidarity and individual development in an organized, specialized society could complement one another, rather than individuality leading to social disintegration. In this way is the contradiction between progress and moral cohesion resolved.

Durkheim draws a number of important distinctions in developing the social theory to establish his view of the moral significance and likely outcome of the advance of the division of labor just described. Social orderings are of two main types: *segmentary* and *organized*. Segmentary society contains social units that are similar to one another and show little functional differentiation. Organized society, by contrast, contains functionally specialized social units embedded in a developed division of labor. This is the distinction Waltz emphasizes and equates to anarchy and hierarchy.

Segmentary society is "mechanical solidary"; that is, its social solidarity derives from "the attraction of like for like" which springs from mutual acceptance of society's common moral framework. Often, this common moral framework, which Durkheim calls the *collective conscience*, takes a religious form, and invariably the collective conscience contains those moral precepts which society insists its members must obey on pain of retribution. Organic society's solidarity derives, as noted, from the division of labor itself, and it is a consequence of the perceived mutual dependence of differentiated social units. Note, then, that a common moral bond holds mechanical society together, while organized society's solidarity arises from what we might call interdependence.

Finally, the moral order of society finds expression in two important types of law: the *repressive* law of mechanical society and the *restitutive* law of organic society. The former imposes punishment to induce conformity to group mores and taboos. The latter, of which contract law is the model, intends to restore a prior state of affairs, to make good the losses a party has suffered. For instance, one is punished for consuming proscribed foods, but one pays compensation for causing damage to property. One of Durkheim's most famous insights—that crime in society serves the social function of reinforcing social solidarity—derives from his understanding of mechanical solidarity and repressive law. Crime and punishment serve to maintain social cohesion by reinforcing the authority of the collective conscience. Because repressive law will dominate in mechanical solidary society, while in organic society restitutive law will do so, there is a way to investigate the moral foundations of society: the scholar determines the nature of explicitly formulated law to infer the type of the underlying moral order.

A major question that concerned Durkheim was how to account for the transformation from mechanical to organic society. After all, the question Durkheim addressed was whether social cohesion could survive the loss of mechanical solidarity. He devotes considerable effort to refuting social contract theorists and utilitarians who see in the division of labor a mere instrument for increasing human happiness, and he rejects the thesis that individual desires can explain social change. To the contrary, he argues, happiness might decrease as the division of labor advances, but it would still advance. Consequently, mutually advantageous contract and exchange do not in themselves produce the deepening specialization observed in modern society. Rather, contract and exchange presume a moral context that makes such relations possible and necessary.[2]

Rejecting the utilitarian explanation for the division of labor, Durkheim argues instead that increasing dynamic density can lead to the effacement of the segments of mechanical society, and once the process of effacement has begun the deepening of the division of labor also begins. Placed in closer proximity and more frequent contact, the similar units of segmentary society find that they are competing for the same resources and duplicating one another's functions. This potential for conflict leads them to differentiate themselves so that competition can continue without social disintegration, and this social fact produces restitutive law (Durkheim 1964, pp. 266–75). Eventually, the segments disappear and organic relations in a specialized division of labor take their place. Most importantly, a new basis for social solidarity replaces the old as mechanical solidarity gives way to organic solidarity.

To sum up, Durkheim develops a theory identifying the conditions for social solidarity and the social orders corresponding to each form of social solidarity. Social solidarity is divided into two main types, mechanical and organic. The conditions supporting them are the collective conscience and the division of labor, respectively. The corresponding social orders are segmentary and organized, respectively. In the former, repressive law will dominate, in the latter, restitutive law. Increasing dynamic density and social volume can lead to competition among segmentary units, their effacement, and ultimately the full development of a division of labor. The most important question in this is, What is the basis for social solidarity and moral consensus?

Waltz and Durkheim

What is the relevance of *The Division of Labor in Society* to Waltz's *Theory of International Politics*? Waltz distinguishes domestic and international structures in terms of Durkheim's typology of mechanical and organic solidarity and the corresponding segmentary and organized orders. He places the international system in the mechanical, segmentary class and asserts that the international system, as a segmentary order, lacks a division of labor, or differences between the units. By contrast, the differentiation of units characterizes hierarchical, organic, domestic society. States are segments, Waltz argues, in that they are alike and are related to one another by juxtaposition, rather than displaying the organic ties of differentiation and mutual dependence found within a society. "The parts of a polity," writes Waltz, "bind themselves together by their differences" (Waltz 1979, 104, citing Durkheim 1964, 212). Conversely, states, inasmuch as they are alike, are not members of the same polity and are merely loosely linked in the international system by their sameness.

Thus Waltz equates mechanical solidary society with anarchy, organic society with hierarchy, and relies on Durkheim's authority for the reasonability of reducing all societies to these two polar types (1979, 115). In addition, he asserts, the governmental function develops in organic systems, and this enables the division of labor to advance unimpeded in domestic society. Waltz writes, "Units previously alike become functionally distinct as some of them take on system-wide tasks" (p. 197)—a proposition for which Waltz cites all of *The Division of Labor in Society*. Because authority allows economic actors to presume their physical safety, the division of labor can advance in domestic society, while, conversely, the absence of such authority in the segmentary international system prevents an international division of labor from advancing too far. Thus power and authority are the prerequisites for the development of a division of labor. Likewise, concentrated power (hegemony) can provide scope for some functional differentiation in an anarchic system international system.

One might ask why Waltz found it desirable to formulate his theory in terms of Durkheim's social theory. According to Ruggie, the gain is Durkheim's "methodological premise" that social totalities are the appropriate units of analysis. This premise lends weight to Waltz's assertion that "reductionist" theory is fundamentally mistaken, an argument that is deployed against the intellectual constituencies mentioned earlier. Waltz is clear that his own thinking owes a lot to Durkheim: "In our discourse, saying who was more faithful to Durkheim is less important than finding the theoretically most tenable and practically useful definition of structure. Durkheim is nevertheless an able guide to the elusive prey" (1986, 323). I agree that the goal is not to be faithful to Durkheim for its own sake. Rather, the reason this debate retains interest is that Waltz's suggestion to employ Durkheim's theory to understand international relations is a good one; but precisely because faithfulness to Durkheim's work provides more insight into international relations than misconstruction, more careful reading of his work is needed. If Durkheim is an able guide, then we would do well to understand his directions correctly. At least two critics have already questioned Waltz's reading.

Criticisms: Ruggie and Ashley

Ruggie charges that Waltz fails to appreciate the generative conception of structure that Durkheim employed (1986, 142). By this, Ruggie means that the "deep structure" of a given social system, rather than merely explaining the differences across units, contains the principles whereby its units are separated or differentiated. In other words, structure generates the units themselves—states, in international politics—and it is only because he assumes this prior process of generation and separation that Waltz can take for granted the "objects" of his theory. Given these objects (states), Waltz can then discuss power balancing, the virtues of inequality, and the like; but he cannot account for the existence or the nature of the units that constitute the system.

Consequently, Ruggie finds that for lack of generative structure Waltz's model lacks a determinant of change—despite the fact that Durkheim does offer one, with the proposition that increasing dynamic density brings about the transformation from segmentary to organized society and deepens the division of labor in organized society. Specifically, Ruggie interprets Durkheim to imply that property rights can undergo a radical transformation under the pressures of dynamic density, as the shift from medieval to modern society shows (1986, 149). Yet because Waltz replaces a potentially generative version of structure with a descriptive one, he has no place in his model for dynamic density, and this in turn leads to his depreciation of the importance of demographic trends, industrial change, and ecological and resource constraints (Ruggie 1986, 150). Finally, like Durkheim, Waltz is said to go too far in excluding unit-level change from his systemic theory. A methodological principle, Ruggie asserts, becomes an ontological one, so that the theory cannot predict change, though it may reflect it (1986, 151).

Waltz defends his interpretation of Durkheim by reiterating his equation of the international system with mechanical society and arguing that Ruggie errs in identifying change from mechanical to organic society with growth in dynamic density (1986, 323–30). In several ways, Waltz asserts that growth in dynamic density ought properly to be thought of as a unit-level process. That is, dynamic density is an attribute of domestic society, not the international system. So the evidence of increasing dynamic density (i.e., evidence of interdependence) describes a unit-level phenomenon. As long as society is segmentary, Waltz argues, no *structural* change has occurred. He concedes that the unit level interacts with the structural level but asserts that nevertheless the continuities of international politics remain: self-help, anarchy, and the ever-present threat of war. Therefore, although unit-level changes may exert considerable effect on international politics, one must not confuse unit-level and structural variables—and structure predominates in this scheme. Structure does not include everything, but it tells us "a small number of big and important things" (Waltz 1986, 329). The main point is that international politics remains anarchic and therefore, Waltz asserts, segmental and mechanical, despite the apparent increase in dynamic density.

Waltz notes with some justification that Durkheim found segmentary society to be an insurmountable barrier to the advance of the division of labor. Only a certain degree of *effacement* of the units would allow the process of transition from mechanical to organic solidarity to begin. Waltz presumes that no significant effacement of the segments of international society—sovereign states—has taken place or will take place. Consequently, he says that Ruggie is wrong to argue that the principles on which the units are differentiated changed from the medieval to the modern era, or that any fundamental change could occur in the future due to increasing dynamic density. Rather, the continued existence of similar units contains dynamic density's effects within the domestic sphere. He argues that the change in property relations that took place in the transition from feudal to modern society represents no structural change, inasmuch as "the change Ruggie identifies does not move international society from a condition in which like units are weakly held together to one in which unlike units are united by their differences" (1986, 326). Thus the system remains mechanical. Waltz also observes that for Durkheim, dynamic density is a moral as well as an economic concept, and he implies that no moral transformation (in the sense Durkheim means) has occurred in the international system. Waltz concludes, "The transformation of social structure is not produced by the mere mutualism of international trade" (ibid.).

The problem with Waltz's reply is that whether dynamic density has effaced or will efface the units in a system is an empirical question. It is not enough to note that Durkheim believes that effacement is required for transformation from the mechanical to the organized form, and this is all that Waltz does. Were such effacement not possible, all societies would remain mechanical and segmentary, and Durkheim would have had nothing to write about. Rather, it remains to investigate whether economic interdependence, population growth, and the like have resulted in "continuous relations" among societies so that one can speak of a "common life." Insofar as this more or less describes the research agenda of regime theorists, liberal institutionalists, and others who examine the effects of global interdependence on the nature of international politics, it is no refutation of those positions to make reference to Durkheim's comments on the necessary conditions for collective existence, as if those comments also prove that no development of such conditions has occurred in world affairs. Just as Ruggie asserts, Waltz takes the unit-structure distinction as an ontological principle rather than a matter for empirical inquiry, thus precluding investigation into the degree of effacement of the units making up the international system.

Waltz, perhaps in response to this objection, attempts to establish that no effacement has occurred in international society, by referring to a passage in Durkheim showing that Durkheim does not expect that international commerce in itself will bring about a division of labor. Rather, as long as humans remain divided into separate societies that transact only occasional business with one another, the result is mere "mutualism" (Durkheim 1964, 282). I shall return to this concept, but for now it is enough to say that Waltz interprets Durkheim to mean that the segmentary form will long characterize international society, because economic interdependence does not necessarily launch separate societies toward economic integration and the organized form.

Furthermore, Waltz argues that if theorists are to avoid the "slippery slope toward reduction" that would allow systems to proliferate wildly with each change in the investigator's interests, they must exclude the forces for change which dynamic density represents from systemic or structural theory (1986, 328). Durkheim, Waltz tells us, did not think of dynamic density as a structural variable, and neither should we. "Dynamic density is a unit-level condition which may burst the bonds of the old system and break its structure apart, [so that] I, like Durkheim, think of unit-level processes as a source both of changes in systems and of possible changes of systems, hard though it is to imagine the latter" (ibid.). In short, all of the foregoing leads to the conclusion that "Ruggie would lower the level of abstraction by adding to structure more information about the characteristics of units and unit-level processes" (p. 330). It is this tendency of international relations scholars to lower the level of abstraction and define structure arbitrarily that Waltz most deplores, a tendency that he calls on Durkheim's authority to combat.

Richard K. Ashley joins this debate on Ruggie's side, noting that the reason Waltz did not consider the problem that Ruggie raises is that the theory is "statist before it is structuralist" (1986, 272–73). In particular, Ashley agrees that Waltz ignores the problem of establishing a generative version of structure, instead taking for granted the existence of states, which then through their coaction produce the structure of the international system—its anarchy and distribution of capabilities. "The clincher in Ruggie's argument," Ashley writes, "is his attempt to show that the sovereign state, so far from creating modern international society, presupposes international society's production of the sociopolitical space within which sovereignty could flourish as the modern concept of international political identity and liberty."

Durkheim Versus Waltz

Apparently, however, the clincher did not clinch. Waltz did not withdraw or significantly modify his interpretation of Durkheim in his 1986 reply. Furthermore, in 1992 he reiterated his argument in regard to dynamic density's role in international theory (1992, 30). One reason the argument did not convince him might be that his critics' point is rather more understandable in terms of neo-Marxist or hermeneutic interpretations that emphasize the historicity of social science concepts than in terms of Durkheim's *Division of Labor*. Be that as it may, I argue that the critique of Waltz's use of Durkheim would certainly strike home were the emphasis placed on what Durkheim had to say about the difference between mechanical and organic *solidarity*, their relationship to law and authority, and the notion of mutualism. For Waltz undertakes an extremely idiosyncratic interpretation of Durkheim's work to make his own, very different point. Whereas Durkheim was concerned to show the moral basis for social solidarity, Waltz seizes on the notion of segmentary society to derive a power-political model of international relations more consonant with Hobbes than Durkheim.

To begin, Waltz confuses the relationship between anarchy and hierarchy on the one hand and mechanical and organic solidarity on the other. Durkheim, he writes, "distinguishes between societies of mechanical and of organic solidarity, corresponding respectively to the anarchic order of international politics and the hierarchic order of domestic politics" (1986, 323). Yet Waltz's attempt to make organic society synonymous with hierarchic, and mechanical synonymous with anarchic, is mistaken. A clue that some conceptual confusion reigns is that Waltz derives the like-units proposition from international anarchy, arguing that the pressure of competition that forces units to look after themselves produces their sameness. They perform similar functions, for to do otherwise risks their elimination. Durkheim, by contrast, asserts that social segments become similar under *moral pressure*, and that competition is the root cause of *differentiation*. Competition leads to an ever-greater division of labor and possibly to organic solidary society, not to the units' likeness or to mechanical society.

Moreover, nowhere does Durkheim say or imply that segmentary society necessarily lacks constituted authority, that mechanical society is necessarily anarchic. In numerous places he says quite the reverse. In particular, inasmuch as similarity among the segments is associated with enforcement of the dictates of the collective conscience, the units' likeness rests on the punitive enforcement of moral rules and taboos, something quite impossible in the kind of anarchic international system Waltz postulates. In Waltz's system, states seek mainly to survive, act out of self-interest, and rarely act together to enforce collectively held beliefs. Indeed, were such beliefs real and were collective action regularly possible in a self-help system, no hegemon would be necessary to sustain cooperative economic arrangements, as Waltz argues. Durkheim, on the other hand, argues that the maintenance of mechanical solidarity often requires an authoritative organ capable of exacting retribution. He writes, "Wherever a directive power is established, its primary and principal function is to create respect for beliefs, traditions, and collective practices; that is, to defend the common conscience against all enemies within and without" (1893, 84). Thus the chief, the king, the priest, the tribunal, and other such authorities mark mechanical society. There is no necessary relationship between anarchy and mechanical solidary society whatsoever.

To be sure, mechanical society need not be hierarchical. Enforcement could depend upon generalized social pressure, so that society remains in a certain sense an anarchic realm. Durkheim notes that the functioning of repressive justice tends to be diffuse and goes on to say that "in very different social systems, it does not function through the means of a special magistracy, but the whole society participates in a rather large measure" (1964, 76). Still, whether authority is centralized or diffuse, the central concept defining mechanical solidary society is the common core of belief represented in the collective conscience, not the presence or absence of constituted authority. The collective conscience links social segments insofar as they accept its basic precepts and make conformity to them the basis for social solidarity, but this comes about whether a central authority enforces conformity or the approbation of society as a whole is brought to bear without need for a constituted ruler. Hence, inasmuch as either diffuse social pressure or a directive body can enforce the collective conscience, it is a mistake to equate mechanical society to anarchy. Rather, mechanical society can be either anarchic or hierarchical, provided social solidarity depends upon the collective conscience.

Waltz touches on the basis for mechanical societies' unity. He ascribes it to the "resemblance of their members," arguing that the units are "loosely linked" by similarity (1986, 324). But mechanical society is not necessarily "loosely linked." It is true that a segment may be severed without disrupting the economic life of the rest of society. Even so, the moral bond represented in the collective conscience can link the segments as strongly as (perhaps more than) the organic ties of the division of labor. Indeed, as indicated, the main problem confronted in *Division of Labor in Society* was, What would continue to link the members of organized society when the strong moral bonds of the collective conscience had dissolved? Durkheim's problem was to show that organic society's solidarity could be at least as strong as mechanical society's, that organized society would not fly apart due to the centrifugal effects of modernization! Thus Waltz is wrong when he asserts that Durkheim sees in mechanical society "similar segments weakly united by their resemblance" (p. 325). Leaving aside the question of the sense in which Waltz would postulate the units of the international system to be linked at all, clearly what Durkheim sees is segments strongly united by the collective conscience, similar because they enforce the collective moral order of society.

Regarding the equation of organic with hierarchic society, by the same token, given that there may be hierarchical mechanical societies, hierarchy and organic society are not synonymous either. Rather, just as mechanical society can be either anarchic or hierarchical, so might organic society be one or the other. At least, nothing in Durkheim indicates that anarchic organized society is impossible. Hierarchical organized society is common enough in practice to need no comment. Whether one could find an actual case of organic anarchy is another matter, but utopian communism or libertarian capitalism would fit the conceptual criteria. Indeed, depending upon how far one believes the international division of labor has advanced, one could call the contemporary international political economy an organic anarchy today. However one decides this, the point remains: there is no warrant to equate organic society with hierarchical structure.

Waltz's mistaken classification here identified arises from conflation of mechanical solidary society with mutualism, mentioned above. In mutualism, there is no collective conscience, and indeed no single society. Durkheim applies the concept explicitly to separate societies: "If, however, in certain cases, peoples tied by no bond, even regarding themselves as enemies, exchange products in a more or less regular manner, it is necessary to see in these facts only simple relations of *mutualism* having nothing in common with the division of labor. For, merely because two different organisms are found to have properties usefully adjusted, it does not follow that there is a division of functions between them" ([1893] 1964, 281–82, emphasis original).

As noted, Waltz employs this passage to support his assertion that economic interdependence does not imply effacement of the units, but he fails to observe that mutualism is distinct from mechanical society. Societies in relations of mutualism do not share the kind of moral relationship that either mechanical solidarity or organic solidarity represents. That is, mechanical solidarity and organic solidarity are attributes of a single society, not of two distinct societies with occasional interactions. Merely because Durkheim denies that simple exchange represents a division of labor does not establish that mutualism is a form of mechanical solidary society. No such equation is warranted in a single line of *Division of Labor in Society*. Given this, Waltz's use of Durkheim's remarks on mutualism to refute Ruggie's analysis regarding mechanical and organic society is beside the point. Rather, mutualism and mechanical solidary society are quite obviously mutually exclusive categories. Thus, although citing this short passage allows Waltz to escape obvious self-contradiction, careful reading underlines the point that mechanical solidary society is not synonymous with anarchy as Waltz describes it.

Now, for Waltz, the significant point is that international systems, mechanical societies, and mutualism all share the segmentary form, that the units are similar. But Durkheim does not assert that mutualism displays the segmentary form. Far from it. Durkheim ties the segmentary form inextricably to mechanical solidarity. Nowhere is it mentioned in relation to mutualism, and Waltz does not attempt to bridge the gap. Furthermore, one ought not to expect the concepts to be linked in that the likeness of the segments of mechanical society is a consequence of moral conformity; rather, it is a consequence of participation in the collective conscience. This source of similarity is nonexistent where the social entities lack a common ground of belief, as they would under conditions of mutualism. To the contrary, rather than mutualism leading to likeness, the only indication of how mutualism affects the units in relation to one another suggests a degree of differentiation—"properties usefully adjusted." Thus, mutualism lacks both the segmentary form and a clear account of the mechanism by which the units would become alike. This difficulty is all the more acute in that Durkheim tells us that competition, far from leading to sameness, ought to lead to differentiation and finally, if normality prevails, to organic solidary society. Thus, if the units are or become alike in a relationship of mutualism, one can only attribute it to random chance.

Furthermore, not even mutualism necessarily implies anarchy. For again, in Durkheim's terms, to

FIGURE 1

Typology of International Systems

		Durkheim's Social Forms		
		Mutualist	Mechanical	Organic
Waltz's Political Systems	Anarchy	Hobbesian state of nature	concert of Europe tribal system	utopianism global economy
	Hierarchy	slavery	theocracy chieftan society	industrial economy

determine whether a relationship is mutualism requires examining not the relations of authority but the character of the moral relationship between the units. Mutualism means simply that no social solidarity exists among the units. Absence of solidarity need not imply anarchy. Relations of domination and subservience can be hierarchical without displaying social solidarity. The African brought in chains to America by traders whose identity and purpose he did not know might testify to this type of "hierarchy." Indeed, any relationship of conqueror to conquered, as long as no moral and social relationship between them forms, can represent hierarchical mutualism. To be sure, the notion of "properties usefully adjusted" would beg the question, Useful for whom?, but the point remains nonetheless: not even mutualism conforms to the anarchy-hierarchy distinction.

In sum, we see that Waltz's use of Durkheim's conceptual framework is in error in several respects. Although Waltz describes the basis for organic solidarity admirably (1986, 324), he ignores the significance of the fact that the main social types that Durkheim posits—mechanical and organic society—differ in terms of their moral basis, preferring to emphasize the consequence that the units in each type are similar or differentiated. He mistakenly equates anarchy with the segmentary form and hierarchy with the organic form, when segmentary and organic society can be either hierarchical or anarchic. To refute Ruggie's proposition that dynamic density can lead to system transformation, Waltz compounds the error by conflating mutualism and mechanical society. Ultimately, the problem with Waltz's formulation is that his purpose is directly contrary to Durkheim's. While Waltz wishes to establish the invariance and continuity of the main features of international life, Durkheim attempts to explain and evaluate fundamental transformations in social relations. Ruggie and Ashley identify this lapse in Waltz's theory, but they fail to clinch the argument insofar as they let pass Waltz's mistaken categories. Such mistakes identified, the weakness of the entire argument, including Waltz's refutation of the significance of dynamic density, becomes obvious.

Observe that I do not oppose Durkheim's typology to Waltz's. Rather, I suggest that anarchy and hierarchy cut across the categories of mutualism, mechanical, and organic society.

A TYPOLOGY OF INTERNATIONAL SYSTEMS

Normal Forms and the International System

What is the significance of this for the theory of international relations? If it is convincing, the best strategy for Waltz and those who find his theory compelling would be to deny the importance of Durkheim to the overall argument made in *Theory of International Politics*. If Durkheim is unnecessary to Waltz's argument, then perhaps others are wrong to assert that international politics has changed or will change structurally. Yet at least three of the most prominent scholars in American international relations have found Durkheim's work to be worth considering in relation to international theory. Perhaps rather than simply drop the matter, it might be possible to employ Durkheim's categories more systematically. If this fails to aid us in understanding international phenomena or to clarify theoretical debates, then we should discard the notion that Durkheim offers anything of relevance to contemporary international studies.

Assuming that Durkheim's categorical distinctions do lend insight to international relations theory, then it only makes sense to employ the concepts properly. To that end, I propose a typology based on Durkheim's categories and Waltz's anarchy-hierarchy distinction, yielding six types of political systems: *anarchic-mutualist, hierarchic-mutualist, anarchic-mechanical, hierarchic-mechanical, anarchic-organic,* and *hierarchic-organic* (Figure 1). Clearly, the types that are of interest to international relations are the three anarchies. Thus an international system could be mutualist, mechanical solidary, or organic solidary, and one might identify subtypes within each of these categories.

Into which of the three anarchical categories does the contemporary international system best fit, and what implications can we draw from that classification? If one wishes to translate contemporary debates into these terms, certain propositions are evident. The nearer one believes an international system is to the Hobbesian state of nature, the more likely one is to see it as mutualism. Lacking common moral precepts, societies might engage in occasional interactions, but they will be ideologically and institutionally divergent and they could be enemies. A likely consequence might be that war and the threat of war will always be near to hand.

If, by contrast, one believes that an international society exists but that states are functionally similar, then the system would be anarchic-mechanical. The basis for social solidarity might be the common understanding that the sovereign state is the proper realm of liberty and political legitimacy. More generally, one might understand the entire framework of international law—including the implicit elements—as the "international collective conscience." Of course, this is far from asserting that the political units in the system become alike as a result of competition. Rather, the members of international society would enforce common standards of institutional and political expression. Their enforcement of these common standards would entail recognizing the sovereign state as the legitimate political expression of national aspiration in world politics. The likeness of the units, then, is a consequence of shared belief, not deadly competition. Lastly, to the extent that one sees an international division of labor, the growth of contract law governing international commerce, and a weakening of the hold of the concept of sovereignty on the international system, one might see the beginnings of international organic solidarity. Regime theory, world order theory, and other forms of internationalism would seem to see the world this way.

Where would Waltz's theory fall in this scheme? Immediately, one can see that the international system Waltz describes must be either anarchic-mutualist or anarchic-mechanical. The distinction is significant, for it rests on whether an international *society* is said to exist. As shown, Waltz shifts from one to the other as the need arises and conflates the two in places (1986, 324, 326), but certain hints indicate that he tends to see the international system as a constituted moral order. For example, in refuting Ruggie, he refers explicitly to "international society" (p. 326). Moreover, he refers to Durkheim's different forms of society so often that he must have sensed that Durkheim had deemed the international system itself to be a sort of society. One has to look diligently to find Durkheim's references to mutualism, yet it is only these that support the conception of amoral relations among individuals or collectivities, thus allowing one to reduce all political life to the struggle for power. Meanwhile, Waltz cannot fail to have noticed, at the same time as the reference to mutualism, Durkheim's assertion on the facing page that "it can be affirmed that an economic or any other function can be divided between two societies only if they participate from certain points of view in the same common life, and, consequently, *belong to the same society*" (1964, 280, emphasis added). This line indicates that Durkheim had no reservations regarding the notion that international society could exist or thus the notion that international relations is an inherently moral realm or that the European state system, for instance, represented a form of society. Waltz's theory would seem to adopt the same view, even though the author intends to offer a structural model based solely on power relations.

Obviously, only Waltz can say what Waltz meant. However, if we are to read those passages in *Theory of International Politics* referring to Durkheim as coherent statements, we must assume the existence of international society of the segmentary form, the segments linked by mechanical solidarity.

Of course, that leaves open for investigation the precise contents of the international collective conscience and the degree of transition to organic solidarity, a largely empirical matter beyond my scope here. Even so, let me offer only a few tentative observations regarding the direction and probable findings of such research. Fortunately, Durkheim has given us some advice on how to undertake empirical investigation of this question. A modern-day Durkheim would no doubt begin to answer the question by asking what type of law exists to regulate the relations among nations, whether predominantly repressive or restitutive. He would investigate the volume and intensity of transactions they share, and he would determine the degree to which these indicate the presence of an organized division of labor. He would certainly wish to know whether dynamic density had begun to efface the segments, setting the stage for the advance of the division of labor and thus organized society. To these questions we can say that in the contemporary international system, states are obviously not isolated entities lacking any semblance of a common moral framework, and evidently an international division of labor has, to some degree, come into being. In other words, the correct course to take, given Durkheim's analysis—and assuming that international society belongs to the normal forms—is to investigate transnational ties and the possible passing away of the segmentary form embedded in the international system. The correct course indicated by our able guide would not be to halt investigation at the proposition that states are autonomous like units. This is to say that, if we can classify the state system according to Durkheim's normal forms, it would appear to be an anarchic mechanical society tending toward organic anarchy.

This raises an interesting question. If the contemporary system is mechanical and the feudal system was mechanical, then in what sense was there a transformation? One would have to conclude that it was from one form of mechanical society to another, which would imply that both Waltz and Ruggie are correct but from different points of view. Interdependence theorists would perhaps argue that the contemporary system is undergoing a transformation at least as profound as the transformation from medieval to the modern system. Rather than the reconstitution of more or less autonomous units on a "higher level," we are today witness to the development of a truly global division of labor, with all that that entails. Returning to the idea of dynamic density, the fact that people, goods, and messages travel across borders at an accelerating rate is beyond refutation. The disintegration of the Warsaw Pact, the signing and ratification of the North American Free Trade Agreement, and the opening of the European Community point toward even greater flows of all kinds. This implies a further deepening of the global division of labor, further "effacement" of the nation-state, and pressure toward greater organization of global society. Thus, in regard to the main question animating this debate, Durkheim's analysis points toward change and transformation in the international system, not to continuity and persistent structural limits on the development of international institutions.

In short, contrary to Waltz's assertions, Durkheim does not provide any greater support for his theory than to liberal internationalism—probably the reverse. Yet looking beyond this debate, my own view of the matter is that the contemporary system does not fit well within the typology of normal forms. Although I tend to side with the interdependence theorists who see a fairly developed and deepening global division of labor, I also bear in mind that Durkheim posits mechanical and organic forms of *social solidarity*. Thus I have to question the relevance of these normal forms to the conflict-ridden world in which we live, where social solidarity is woefully lacking. In that vein, I shall argue that to categorize the contemporary international system, we need to turn to another aspect of Durkheim's work, his analysis of abnormal forms of society.

Abnormal Forms and the International System

So far I have discussed only Durkheim's theoretical work on the normal forms of society. Yet Durkheim is most famous for his propositions regarding abnormal forms of the division of labor, especially those relating to the idea of *anomie*. To consider whether we would better understand international society as an abnormal form presumes that there is some level of an international division of labor and an international society. That implies that the contemporary international system is not mutualist or segmentary. It must, if it is one of the abnormal forms Durkheim analyzes, be an organized society. Durkheim did not discuss abnormal forms of segmentary society, but nothing precludes their existence. For those who believe that the international system remains segmentary, development of a typology of abnormal segmentary forms would be in order.[3]

Durkheim discusses three abnormal forms of the division of labor: the *anomic* division of labor, the *forced* division of labor, and the *uncoordinated* division of labor. The anomic division of labor is the absence of effective regulation, usually the result of inadequate development of the regulative organ, government. Consequently, one observes commercial crises, capital-labor conflict, and chaos in intellectual life (1964, 354–56). More significantly, alienation plagues individuals who are excessively specialized and disconnected from society. Thus, although the division of labor advances, social solidarity suffers: "The diversity of functions is useful and necessary, but as unity, which is no less indispensable, does not spontaneously spring up, the care of realizing it and of maintaining it would constitute a special function in the social organism, represented by an independent organ. This organ is the State or government" (p. 358).

That the organ of government is ill developed in international society goes without saying. This fact is the very basis of the discipline of international relations. Equally pertinent, Durkheim explores in detail the causes of anomic society, and he finds that a primary cause is the growth of the world market. The passage is worth citing, for it goes to show that the division of labor can extend beyond the bounds of a single political unit. Indeed, that is the very source of crisis.

> In so far as the segmental type is strongly marked, there are nearly as many economic markets as there are different segments. Consequently, each of them is very limited. . . . On the contrary, as the organized type develops, the fusion of the different segments draws the markets together into one which embraces almost all society. This even extends beyond, and tends to become universal, for the frontiers which separate peoples break down at the same time as those which separate the segments of each of them. The result is that each industry produces for consumers spread over the whole surface of the country or *even of the entire world*. Contact is no longer sufficient. . . . Accordingly, production becomes unbridled and unregulated. It can only trust to chance, and in the course of these gropings, it is inevitable that proportions will be abused, as much in one direction as in another. From this come the crises which periodically disturb economic functions. (1964, 369–370, emphasis added)

Clearly, then, Durkheim anticipated a global organic society, a fact that plainly refutes the proposition that segmentary society would stand as an insurmountable barrier to such global differentiation. More to the point, he anticipated that it would suffer severe problems because of its anarchic political organization. The remedy—a world state—is not likely to result soon. Hence, the abnormal anomic condition of global affairs is likely to persist and worsen. Given that there is an international division of labor, then, classifying its current state as abnormally anomic in Durkheim's sense appears reasonable. If there is a reason not to classify the contemporary system as an anomic division of labor, it is that the role of force remains so important in international life. This fact is not reflected in the description Durkheim offers of the anomic division of labor.

Besides the well-known anomic form, Durkheim analyzes the uncoordinated division of labor. To illustrate, a badly managed business creates idleness and waste, "solidarity breaks down, incoherence and disorder make their appearance" (1964, 389). This is not the same as the anomic form, for merely establishing a regulative organ cannot resolve it. Regulation must be applied in a certain way, so as to reduce unnecessary work and increase efficiency. Otherwise, work becomes discontinuous, periods of inactivity punctuate it, and the product is insufficient. Accordingly, solidarity declines. Smoothly functioning, work becomes more continuous. Because an increase in activity of function implies an increase of solidarity, the improvement of one is the condition for the improvement of the other.

In international economics, the parallel case might be the periodic local slumps that result from the fluctuations in the world market and the world production process. Underproduction alternates with overproduction, so that nations slow and speed their activity, always seeking an unattainable equilibrium with external conditions. Developing nations' desire for commodity price stabilization schemes could be a response to this source of instability and may indicate that this abnormal form finds expression in world politics. In any case, Durkheim discusses the uncoordinated form but briefly. The only solution even implicit in his discussion is better planning. Whether such an effort on a global scale would be preferable to the relatively unregulated international market is debatable. However that may be, the form could apply to international relations, but again, the role of force is underemphasized.

The abnormal form to describe contemporary international relations most aptly is Durkheim's notion of the forced division of labor. The anomic and the uncoordinated division of labor do not capture the overwhelming importance of physical coercion and the distorting effects of military competition as well as does this form. By contrast, in the case of the forced division of labor, it is the application of power, not the lack of it, that is the source of the evil. In domestic society, rigid enforcement of rules holds individuals in particular ranks, roles, statuses, and caste positions, while whole nations suffer the same in international society. Durkheim notes that in domestic society, strictly regulated organization, as is found in caste systems, can create dissension, and can result in civil war. The root of the problem is enforced inequality, which entails that the distribution of functions "no longer responds to the distribution of natural talents" (1964, 375). Likewise, in international society, international law becomes a thin tissue for legitimating the will of the stronger and for maintaining systems of rank and status that derive from prior wars, economic exploitation, and racial and religious discrimination. Accordingly, shifts in the resources of different nations in a global organized society do not lead to commensurate shifts in prestige or reward. This, then, becomes a root cause of dissension and conflict.

Contrast this view with Waltz's. Waltz argues that concentrated power is prerequisite to international order. Anarchic orders require a benevolent dictator for even a modest division of labor to develop (Waltz 1979, chap. 7). Durkheim, by contrast, although he might favor a governing power to overcome anomie, argues for the virtues of equality. For concentrated power can stand in the way of the spontaneous development of the division of labor, and the result is the abnormal form here described.

We can see the forced division of labor especially in the relations between rich and poor nations. Changes in the aspirations, economic capabilities, and the value of natural resources have not produced a commensurate change in the nature of international relationships. The North dominates the South. Military dominance and control over the levers of economic power ensure that the poor nations are held in place in a sort of international caste system. Durkheim writes:

> We may say that the division of labor produces solidarity only if it is spontaneous and in proportion as it is spontaneous. By spontaneity we must understand not simply the absence of all express violence, but also of everything that can even indirectly shackle the free unfolding of the social force that each carries in himself. It supposes, not only that individuals are not relegated to determinate functions by force, but also that no obstacle, of whatever nature, prevents them from occupying the place in the social framework which is compatible with their faculties. (1964, 377)

Quite clearly, the existence of great inequalities of national power and wealth imposes direct and indirect limitations on this spontaneous adjustment to the natural distribution of talent. Likewise, the struggle for international power saps resources and lives into a forced division of labor of massive proportions. Military competition reinforces the regimentation of society and stifles the free development of creative abilities. That is, numerous direct and indirect barriers to the "free unfolding of the social force that each carries in himself" exist because the political organization of the world in terms of unequal sovereign states locked in deadly competition is incompatible with the emerging organized global society.

Thus Durkheim's work can provide insights for the theory of imperialism. Waltz criticizes that theory for its supposed slipperiness of definition, which allows the theorist to see capitalistic imperialism wherever inequality exists. Perhaps the attempt to place a Marxist gloss on the domination of the poor nations by the rich does have its problems, but the same point can be made without strain in Durkheim's terms. To the extent that the state system serves the interests of the rich by imposing and maintaining certain political forms and relationships by force, it acts as a direct and an indirect limitation on the free development of individuals and societies the world over. The insidious forms of imperialism modern Marxists have identified, although removed from direct economic exploitation or domination, nevertheless limit opportunity, undermining the advance of the division of labor and social solidarity. Cultural imperialism, racism, and the imperialism of free trade tend toward the same result, even though by indirect routes. In short, if one wishes to invoke Durkheim's authority, one can do so just as well in support of neo-Marxism as of neorealism.

To conclude, if Durkheim offers anything to scholars of international relations, the most relevant aspect of his work is the insights found in the analysis of abnormal forms. It is assuming far too much to postulate that the international system is in any sense normal, an assumption that one must make if the categories of social solidarity described earlier are to apply. Further, I dispute that Durkheim's work would lead to such categorization. Even were Waltz correct to label the international system as segmentary—a matter for empirical investigation, not theoretical presupposition—it would remain to develop a typology of abnormal segmentary societies. For it would be foolish to argue that the contemporary system displays any great degree of social solidarity. The reason it does not is plain: social solidarity requires relations based on moral consensus and fair dealing. It does not ensue from domination. What, then, might produce international social solidarity? If social solidarity is to exist, Durkheim tells us that we must remove the external sources of inequality. So Durkheim can be our authority that "just as ancient peoples needed, above all, a common faith to live by, so we need justice" ([1893] 1964, 388).

Notes

1. Although my purpose is to focus narrowly on Waltz's particular use of Durkheim, some scholars have recently extended, by way of the concept of *structuration,* the Durkheimian tradition to international relations (see Adler 1991; Wendt 1992). For a more direct application of Durkheim to international relations, see Larkin 1994.

2. This insight of Durkheim's bears directly on current debates over the usefulness of rational choice theory and microeconomic analogies in international relations. Durkheim's work would seem to oppose such approaches, which in turn renders problematic Waltz's turn to microeconomic reasoning for theoretical inspiration.

3. The notion of *abnormal* forms of society rests on the proposition that a *normal* society is one in which some kind of order, justice, and peace predominate. Obviously, this is hardly characteristic of the international system, which is good reason to think that Durkheim would see international society as abnormal. *Abnormal,* then, is not to be understood as indicating the (statistically) exceptional condition of society, in that disorder, inequality, and violence are regularities of international life. Thus, although Durkheim vaccilates between a statistical and a normative view of normality and abnormality, his concern for the moral basis for social solidarity indicates far more concern for the normative definition than the statistical.

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THE CONSEQUENCES OF NEGOTIATED SETTLEMENTS IN CIVIL WARS, 1945–1993

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We know very little about how civil wars end. Harrison Wagner has argued that negotiated settlements of civil wars are likely to break down because segments of power-sharing governments retain the capacity for resorting to civil war while victory destroys the losers' organization, making it very difficult to resume the war. An analysis of a data set of 91 post-1945 civil wars generally supports this hypothesis but only in wars over identity issues. Moreover, while military victories may be less likely to break down than negotiated settlements of identity civil wars, they are also more likely to be followed by acts of genocide. Outsiders concerned with minimizing violence thus face a dilemma.

As the threat of thermonuclear extinction declines, our concern with internal conflicts increases. With the ending of the Cold War, great powers have less stake in ensuring that "their side" wins and more stake in peaceful settlements. There is a substantial literature on the *causes* of such violence, an important topic. However, from the point of view of outsiders, the *ending* of such violence is often the major consideration, whether for reason of policy or humanitarianism; but ironically, we know much less about how this happens, with or without outside assistance.

This project is part of a research enterprise designed to help find out how to facilitate the end of civil violence. Nothing is said here about justice, about whether a policy of war termination is *appropriate* toward any state or group of states, about who will benefit or suffer from it. The ending of violence may well not be the goal of concerned outsiders in any particular situation. While they will hopefully not support violence for its own sake, they may certainly not wish to encourage a civil war to end on terms that they think are inappropriate (e.g., genocide for much of the population, a nondemocratic government, repression of one group by another, adoption of policies they dislike). This research rests on the normative assumption that at some times we will want to know the conditions under which civil wars are more or less likely to end. In particular I am concerned with the *consequences of ending civil wars with negotiated settlements or military victories.*

A number of authorities argue that civil wars will be more difficult to end than interstate wars. Interstate opponents will presumably eventually retreat to their own territories (wars of conquest have been rare since 1945), but in civil wars the members of the two sides must live side by side and work together in a common government after the killing stops. Compromise is particularly difficult because the stake is control of this new government and is thus, literally, life and death for the combatants (Bell 1972, 218; Iklé 1971, 95; Modelski 1964, 125–26; Pillar 1983, 24–25; Zartman 1993, 1995).

This leads us to expect civil wars to be both more *intense* and therefore more *difficult to resolve by negotiation* than interstate wars. There is some support for both hypotheses. Hugh Miall's data show that 15% of international wars involved major violence, while a full 68% of civil and civil/international wars did (1992, 124). Paul Pillar's data show that about two-thirds of interstate wars but only one-third of civil wars ended by negotiation (1983, 25). Using a somewhat modified version of Pillar's data, Stephen Stedman found that when colonial wars and other "special" cases were eliminated, the percentage of civil wars settled by negotiation declined to about 15% (1991, 9).

The implications of these findings are ambiguous. On the one hand, casualties do seem to be higher and settlements more difficult in civil wars, suggesting that there may be interesting theoretical differences between the different sorts of violence. On the other hand, while suppression, genocide, or partition are certainly possible, negotiated settlements to civil violence do in fact occur: the cell is not empty.

Sustained wars produce and reinforce hatred that does not end with the violence. How do groups of people who have been killing one another with considerable enthusiasm and success come together to form a common government? How can you work together, politically and economically, with the people who killed your parents, siblings, children, friends, or lovers? On the surface it seems impossible, even grotesque.

But in fact we know that it happens all the time. Fred Iklé's apt title, *Every War Must End*, applies to civil wars as well as interstate ones. The mass violence stemming from Catholic-Protestant religious differences within Western Europe several centuries ago has ended. England is no longer crisscrossed by warring armies representing York and Lancaster or King and Parliament. The French no longer kill one another over the divine right of kings. Americans seem agreed that they will be independent of English rule and that the South should not secede. Argentines seem reconciled to living in a single state rather than several. The ideologies of the Spanish Civil War now seem irrelevant, and even the separatist issues there are not being resolved by mass violence. Nigeria experienced one of the most brutal civil wars of our time, but the violence ended 20 years ago, and while the country is politically unstable, the divisions are not the same as in the civil war. In Colombia, a power-sharing agreement in 1957 facilitated an end to *la violencia* and was followed by 15 years of relative peace during which old rivals traded control of the government in a process resembling a quadrille. A settlement between the combatants in North Yemen produced a regime sturdy enough to merge with South Yemen some years later and, more recently, to wage with success another civil war to oppose southern secession. Somehow, effective new societies were constructed after these wars, involving most of the people who had fought on opposite sides.

How did this happen? We know very little about the processes involved. I shall explore one particular aspect of this process, the consequences of negotiated settlements and military victories for renewed violence.

DEFINITIONS AND RESEARCH DESIGN

This line of reasoning also suggests that the conventional definition of a civil war—large-scale violence between groups holding sovereignty within a recognized state—is inadequate. The problem is the notion of *recognized state*, which is essentially a legal criterion. However, conflict increasingly seems to pay little attention to the distinction. Is it appropriate to see the Palestinian Intifada as not a civil war because Israel is an occupying force rather than a recognized sovereign in the territories? The war in Zimbabwe from 1972 to 1980 can be viewed as a colonial struggle, since there was no legitimate government in place before the end of the war, but in fact, the fighting on both sides was carried out by local residents, not troops from a colonial homeland. Whether Bosnia was a state before it was torn by violence is an interesting question, but it begs the more significant issues of why the violence has been so intense, how it is likely to end, and what its consequences will be.

Iklé's argument suggests that the particular intensity of a civil war stems from the *nature of the stakes*—from the expectation that after the violence, regardless of the results, the participants will have to live together in the state which is being shaped by the war. Thus the term civil war is applied to any conflict that satisfies *all* of the three following criteria:

1. Some influential leaders must be concerned about possibly having to live in the *same political unit* with their current enemies after the killing stops. This concern must be important enough to influence the kind of settlement they are prepared to accept.
2. There must be *multiple sovereignty*, defined by Charles Tilly as the population of an area obeying more than one institution. "They pay taxes [to the opposition], provide men to its armies, feed its functionaries, honor its symbols, give time to its service, or yield other resources despite the prohibitions of a still-existing government they formerly obeyed" (Tilly 1978, 192). This criterion differentiates civil wars from other types of domestic violence, such as street crime and riots, in which there is no centralized control of the opposition. To distinguish civil wars from colonial wars, each side must have significant numbers of troops made up of local residents.
3. A civil war, by our definition, involves *large-scale violence*, killing people. I used the operational definitions of the Correlates of War project: (a) 1,000 battle deaths or more per year and (b) effective resistance, that is, at least two sides must have been organized for violent conflict before the war started or else the weaker side must have imposed casualties on its opponent equal to at least 5% of its own (to distinguish between civil wars and political massacres) (Small and Singer 1982, 214–15).[1]

Several implications of this definition are worth noting explicitly. First, it includes both *revolutions* (a war for the control of the central state apparatus, regardless of the degree of social transformation sought) and wars of *secession*. Indeed, it may even include events often called *coups* (Argentina and Bolivia) if they meet the criteria. Second, international involvement is not included in the definition. While such involvement may indeed be critical in determining the course of the war, its presence or absence is irrelevant in determining its *existence*. If significant numbers of inhabitants of the area are combatants on *both* sides, the violence is classified as a civil war, regardless of the level of involvement of other actors.

Thus the Palestinian uprising satisfies the first condition (since Arabs and Jews are likely to cohabit some states, regardless of the outcome) but is excluded because its death count is too low. The South African riots in 1976 met the first and third criteria but had insufficient central control to be classified as a civil war.[2] Zimbabwe is included because the issue of the war was the nature of the government that would control the state in which most of the combatants expected to reside and because the combatants were primarily local on both sides. Indeed, this definition would include wars of *conquest*, where one group tries to incorporate another into the same state, but very few of them can be found after 1945.

But what does it mean to say that a civil war *ends*? It is commonplace, for example, to say that the American Civil War has not yet ended, that the struggle between differing cultures in the North and South continues to this day in various ways. But there is a distinction between *war* and *conflict*. The distinguishing qualities of *war* are multiple sovereignties and violence, as set forth above. The *underlying conflict* which triggered the violence may well continue, but if (1) the concern about living together, (2) the multiple sovereignty, or (3) the violence ends, the civil war ends for purposes of this analysis.

These issues were first explored in a series of six case studies of large-scale civil wars after 1945 that had been *ended for at least five years*: Greece, Colombia (1957), North Yemen, Nigeria, Sudan (1972), and Zimbabwe. The American Civil War was also included. The studies were written by country experts in response to a framework paper (Licklider 1988). After the first drafts had been written, the authors were brought together with specialists in war termination at a conference sponsored by the U.S. Institute of Peace and Rutgers University. The framework was greatly simplified, the cases were revised, and several theoretical papers were also written to produce an edited book (Licklider 1993).

The seven cases are remarkably varied in a number of aspects, which makes them particularly useful for theory construction. They include three negotiated settlements (North Yemen, Sudan, and Colombia); two total victories (Greece and Nigeria); and two mixed examples of military dominance combined with negotiation (more negotiation in Zimbabwe, less in the United States). Some of the wars were separatist (Nigeria, the United States, and Sudan); others were revolutions (Greece, North Yemen, Colombia, and Zimbabwe). There was extensive outside intervention in North Yemen and Greece; somewhat less in Nigeria, Sudan, and Zimbabwe; and much less still in Colombia and the United States. Their only apparent shared characteristic is that large-scale civil violence ended for a considerable time after the settlement.

However, despite this diversity, they were not a random sample of the Small and Singer data, which itself is legitimately subject to question. They were designed to be used for *theory construction*, but they were not appropriate for *theory testing*.

To remedy these problems a data set of 91 civil wars (see Appendix) was constructed. These data are intended to include all civil wars from 1945 to 1993 (inclusive) that fit my definition and thus provide a stronger basis for generalizations than the seven cases. They were created by applying these definitions to the conflict data sets of several other authors, primarily Small and Singer (1982), Pillar (1983), Stedman (1991), Sullivan (1994), Brogan (1990), Maill (1992), and Sivard (1992, 1993). In general these sources agreed with one another on particular episodes. When they did not, the source reporting longer duration and higher casualty figures was generally selected. In a few cases particular information on specific wars was used. (The codings for Burma and Algeria B, for example, rely on M. Smith 1991, 88–101 and Spencer 1994, respectively.)

This data set is fairly primitive. The number of cases is quite small, even when the entire population rather than a sample is analyzed. Many of the variables are nominal, and the reliability of some of the figures is questionable. I do not believe this data warrants the use of sophisticated statistical techniques. Instead, I simply report Pearson's chi-squared, the probability that the two variables are independent of one another, and associated statistics. Even these are handicapped by the small expected values in some cells, but at least they give us some sense of the likelihood that the relationships have occurred by chance.

HOW DO CIVIL WARS END?

Do civil wars indeed end at all? We can all recite examples where internal violence seems endless, going on for decades or longer—Lebanon, Cambodia, Burma, East Timor, and so on. I. William Zartman (1993, 1995) makes a powerful argument that a likely outcome of internal wars is de facto secession, where each side has effective, unchallenged control of a territory and population. Most uprisings will be crushed, but for many others a kind of balance emerges. The government will have more resources than its opponent, but the rebellion is not the only—or even the most important—item on its agenda. The rebels compensate for lack of resources with intense commitment.

Often a military stalemate and a rough kind of equality emerge. In an interstate war this might provide the basis for some sort of negotiated settlement. However, in an internal war the government abandons its legitimacy if it openly negotiates with the rebels. It is easier simply to withdraw from part of the country (usually to the capital) and continue normal life. The commitment of the rebels makes them equally unwilling to abandon the struggle, which has become their life, even if they could get government agreement to their demands and some guarantees that the agreements will be carried out after they disarm, which is difficult. They too find it comfortable simply to establish their own government in a limited area indefinitely.

The Zartman (1993, 1995) argument is persuasive, and it clearly describes a number of his examples quite well in the short run. However, it has at least two major problems. First, this sort of de facto secession does not really seem to be a final result. Both sides may find stalemate more attractive than negotiation, but neither is usually prepared to accept it as a long-term solution. Thus the level of violence is likely to stay fairly high, although with considerable variation.

A more serious problem is that it is unclear when de facto secession may *not* apply—when settlement of some sort may become more attractive than stalemate. Put differently, Zartman's argument implies that *no civil war will ever end*. But in fact, this does not seem to be true.

Aside from the obvious historical examples mentioned, more systematic analysis suggest that modern civil wars do indeed end. Small and Singer list 44 civil wars from 1940 to 1980, only 4 of which (Philippines, Angola, El Salvador, and Afghanistan), or 10%, were classified as still in progress at the end of the period (1982, 222). Stephen Stedman (1991), drawing on work by Paul Pillar, Quincy Wright, and Small and Singer, developed a list of 47 civil wars after 1945 and said that only 3 (Angola, Cyprus, and Lebanon), or a bit less than 10%, were still going.

My data generally confirm this pattern. Of the 91 civil wars, 7 (South Africa II, Nicaragua B, El Salvador, Ethiopia, China C III, Morocco, and Mozambique) were discarded because violence ended there less than five years before 1994. Thus they were not ongoing, but it was too early to call them ended. Of the 84 remaining, 32% (27) are classified as ongoing in 1994. This figure is higher than earlier studies, reflecting the outbreak of civil violence following the end of the Cold War.

The data also show that most civil wars do not last very long. Of the 84, 27% ended during the same calendar year in which they began; we can call these *abortive* civil wars. Over half ended within 5 years. By 10 years, the figure is almost two-thirds. Malaysia, Laos, and Vietnam ended between 11 and 20 years from when they started, and China A lasted 22 years before ending. Of the 27 ongoing civil wars in 1993, 14 had already lasted over 10 years. Therefore, if we define *protracted* civil wars as those lasting over 10 years, 21% of all civil wars (18, i.e., 4 concluded and 14 ongoing) fall into this category.

Protracted *conflicts* thus may well be fairly common; protracted *civil wars* are somewhat less so. Zartman's argument, then, goes a long way toward explaining the *difficulty* of ending such wars, but we return to our original question: Given that some (all?) civil wars *do* end eventually, how does this happen, and what difference does it make?

As noted, reasonable argument backed by evidence suggests that civil wars are very difficult to end through negotiation because the stakes are so high and because (as in interstate war) no institution can be trusted to enforce agreements. Indeed, this proposition is one of the major analytic justifications for studying civil wars separately from interstate wars.

Those wars which have ended were divided rather crudely into (1) those which ended with a military victory of one side and (2) those which ended in some sort of negotiated settlement (defined as an end to the violence reached while both sides had significant military capabilities remaining and therefore presumably could have decided not to stop fighting if the terms were unacceptable). The data suggest that most civil wars are ended by military victory but that negotiated settlements are a regular phenomenon. Of the 57 civil wars which have ended, one quarter (14) ended by negotiation, while the remaining 43 ended in military victory.

NEGOTIATED SETTLEMENTS IN CIVIL WARS AND THE WAGNER HYPOTHESIS

Given that negotiated settlements do occur, what are their consequences? In particular, are they efficient in bringing peace? Many outsiders regard a negotiated settlement as a good way to end civil violence, for at least two reasons: (1) it will result in *fewer casualties* than the alternative of military victory for one side, and (2) because it involves some sort of *power sharing* among the two parties, it is likely to make future violence among the parties less likely. In fact, however, neither conclusion is intuitively obvious.

Reducing Casualties

The easiest way to reduce casualties in civil war is for one side to surrender and for the other side not to carry out genocide. This is the usual result; most of the abortive civil wars end in victory without genocide.

The more interesting question is whether civil wars which have lasted for longer than a year will have fewer casualties if resolved by negotiated settlements or by victory. There are theoretical arguments on both sides, because it depends on the counterfactual, What would have happened otherwise? Presumably the short-term casualties will be higher for negotiated settlements if the alternative is that one side surrenders immediately with no subsequent genocide and will be higher for military victory if the alternative is that the war is prolonged by the loser fighting longer because of despair of the alternative or if the winner resorts to genocide after the war.

Casualty estimates in civil wars are notoriously inaccurate, so I measured them by orders of magnitude: 1,000 to 10,000 deaths, 10,000 to 100,000, 100,000 to 1,000,000, and over one million. A similar approach has been applied by Lewis Richardson and Hugh Miall. Using these categories, casualty patterns are similar for wars that end in negotiated settlements and military victories.

Sharing Power

The problem of civil war termination is not simply stopping the killing. If the people involved will continue to live in the same state (and these data show only five cases of political separation after civil war between 1945 and 1994), the more interesting problem is creating a state in which former enemies can work together—if not in love, at least without again resorting to mass violence. (For preliminary discussions of this process in terms of state formation and collective memory, see Licklider 1992, 1995.) Negotiated settlements presumably involve power sharing (otherwise it is hard to see why both sides should accept them), which seems a promising way to begin the formation of a state that can handle the underlying problems that caused the violence in the first place.

However, a number of practitioners have suggested informally that negotiated settlements of civil wars are more likely to break down into large-scale violence than are military victories. R. Harrison Wagner (1993) reaches a similar conclusion with a much more sophisticated analysis, arguing that negotiated settlements are likely to create internal balance-of-power situations that make it difficult for the new government to function effectively. Military victory, on the other hand, will destroy the organizational structure of one side, making a resumption of the civil war much more difficult. We can call this the Wagner hypothesis and express it formally: Negotiated settlements of civil wars are more likely to break down than settlements based on military victories; consequently, the long-term casualties of negotiated settlements are likely to be greater than those of military victory. (For similar arguments about interstate wars, see Blainey 1988, 109–24 and Maoz 1984.)

Wagner's argument is that without an organizational structure, an opposition will have great difficulty in starting a civil war; this can be called the *repressive* explanation. An alternate argument is that any state that experiences civil war probably has major internal problems but that those problems are unlikely to be resolved unless the postwar government can undertake structural change at the expense of vested interests. A negotiated settlement to a civil war is likely to result in veto groups that will not surrender power for social change whose impact on them is uncertain; but a military victory will destroy the power of such groups, making renewed conflict more difficult and allowing the government to act as it pleases. This *social change* explanation does not assume that all such governments will try to resolve underlying social problems, only that they will be more capable of doing so if they choose.

The Wagner hypothesis is generally confirmed by the data. The 57 civil wars that have been ended for more than five years were coded as to whether there broke out in the country, more than five years after the settlement and before 1994, (1) a civil war with the same sides and issues, (2) a civil war involving different sides or issues, or (3) large-scale violence that was not a civil war. The 11 cases involving the latter two outcomes were removed from the analysis because their implications for the Wagner hypothesis were ambiguous.[3]

Table 1 shows that three-quarters of these cases (34 of 46) were not renewed during the time of the study. Such a renewal occurred in only 15% of the victories, as opposed to 50% of the negotiated settlements. Several different chi-squared measures show this relationship to be significant at the .05 level or better. Interestingly, among those settlements which do break down, military victories do not last as long as negotiated settlements, but the difference is not significant.

Presumably a civil war that recently ended is less likely to break down than one that ended some time ago. Thus, if there were more military victories late in the period, these findings would be suspect. However, victories and settlements have roughly the same pattern over time. Indeed, if anything, victories are somewhat more likely to occur early (44% of all victories occurred before 1960, as compared to only 29% of negotiated settlements), although these relationships do not approach statistical significance. Our data thus generally confirm the Wagner hypothesis that negotiated settlements of civil wars are less likely to endure than the results of military victories.

TABLE 1

Method of Termination and Wars after Settlement

	WARS AFTER SETTLEMENT (%)		
TERMINATION	NO WARS	SAME WARS	N
Military victory	85	15	34
Negotiated settlement	50	50	12

Note: Pearson's chi-squared = .01; Yates continuity correction = .04; likelihood ratio = .02; Fisher Exact Test (one-tailed) = .02; 1 of 4 (25%) cells with expected frequency less than 5.

ARE IDENTITY-BASED CIVIL WARS DIFFERENT?

Much of the recent concern for civil violence is based on struggles between competing identity groups. Current wisdom sees such wars as more difficult to resolve than violence motivated by political-economic issues because they provoke deeper levels of commitment, are more intense, and are therefore harder to resolve by compromise—more broadly, because behavior can be changed more easily than identity (Gurr 1990, 96; A. Smith 1986; Wedge 1986, 56–57). On the other hand, Burton argues that identity issues are easier to resolve because they often involve symbolic issues that can be resolved fairly inexpensively (it does not take much more money to fly two flags rather than one) and because, in the long run, security for one group will produce security for others as well (1987, 42). We can therefore reasonably ask whether the Wagner hypothesis applies to wars involving identity issues.

If identity wars are more intense than others, they would presumably be more likely to be ongoing, last longer, have higher casualties, and be more likely to involve genocide. In turn, as a result of all these factors, it should be more difficult to end these civil wars with *negotiated settlements* than it is to end other civil wars. What do our data tell us about these hypotheses?

Civil wars were first divided rather crudely into those primarily driven by ethnic-religious-identity issues and those driven by other concerns (primarily socioeconomic). Of the 91 civil wars, 69% (63) were classified as identity wars. Identity civil wars are not exclusively a product of the end of the Cold War, although they have increased recently, from about half of all civil wars starting in the 1940s and 1950s to about three-quarters thereafter.

Again discarding the seven cases which ended less than five years earlier, 37% of the 59 identity wars were still ongoing in 1994 as compared to 20% of the 25 others, as the conventional wisdom would predict. However, identity wars do not last longer; of the 57 civil wars which had ended, 5% of the 37 concerned with identity lasted longer than 10 years, as compared to 15% of the 20 political-economic wars, and the differences were not statistically significant.

In general, identity and political-economic wars do not have clearly different casualty patterns, and the divergences are not linear. Nonidentity casualty figures are slightly more concentrated at the extremes, either in the thousands or millions, with identity civil wars more commonly scoring in the tens or hundreds of thousands.

Another measure of intensity is taken from Barbara Harff's data set on genocide. She distinguishes between two different types of political mass murder: "By my definition, genocides and politicides are the promotion and execution of policies by a state or its agents that result in the deaths of a substantial portion of a group. In genocides the victimized groups are defined primarily in terms of their communal characteristics. In politicides, by contrast, groups are defined primarily in terms of their political opposition to the regime and dominant group" (1992, 27–28). I have dichotomized this information, classifying each civil war whether Harff reports genocide and/or politicide for it or not and whether before, during, or after the war. The resulting indicator is labeled *genocide* (my definition of the term being broader than Harff's). From the 91 civil wars, the 7 that began after 1990 are excluded, because Harff's list ends at this point; this leaves 84 wars, including those which are ongoing. Of the 59 identity civil wars, 25% were associated with genocide, as compared to 20% of the 25 other wars; the difference is in the expected direction, but it does not approach statistical significance. Genocide casualties are actually heavier among nonidentity wars; 60% of the 5 political-economic genocides involved casualties of over a million, as compared to only 13% of the 16 identity genocides. (The Pearson chi-squared figure is .04, but the Yates continuity correction is only .14.)

On our five measures of intensity (continuance, length, casualty patterns, recurrence, and genocide), despite the conventional wisdom, *identity civil wars are not clearly more intense than nonidentity ones*. The differences that do exist tend to be fairly small, as are the number of examples. It is therefore not entirely a surprise that *identity and political-economic civil wars are about equally likely to end in negotiated settlements* (26% and 20%, with Ns of 37 and 20). It is apparently no more difficult to reach a settlement of identity civil wars than others. In particular, negotiated settlements can indeed be reached in civil wars focusing on identity issues, despite the high level of hostility involved.

None of this analysis, however, tests the Wagner hypothesis specifically for identity civil wars that negotiated settlements will be less likely to hold than military victories. To put it differently, if negotiated settlements are *possible* ways to end identity civil wars, are they *effective* at resolving the conflicts? Table 2 suggests that they are not. On the contrary, *negotiated settlements of identity civil wars are less likely to be stable than military victories*. Seventy-nine percent of the identity wars that ended in military victory were not followed by violence until 1994, as compared to only 33% of identity wars that ended in negotiated settlements, and the chi-squared statistic is .01. The Wagner hypothesis seems to hold for identity civil wars.

TABLE 2

Issue, Method of Termination, and More Wars after Settlement

	WARS AFTER SETTLEMENT (%)		
TERMINATION	NONE	SAME	N
Identity civil wars[a]			
Military victory	79	21	24
Negotiated settlement	33	67	9
Political/economic wars[b]			
Military victory	100	0	10
Negotiated settlement	100	0	3

Notes: [a]Pearson's chi-squared = .01; Yates continuity correction = 1.0; likelihood ratio = .01; Fisher Exact Test (one-tailed) = 1.0; 1 of 4 (25%) cells with expected frequency less than 5.
[b]Chi-squared statistics cannot be calculated because there is only one column that is not empty.

At first glance, the policy implications of these data would seem obvious: if you want to end an identity-based civil war, encourage a military victory, presumably by supporting the strongest side. This is not an easy prescription to accept. Outsiders will not necessarily find justice exclusively in the side that is winning. Indeed, as in former Yugoslavia today, many may believe that supporting winners may mean opposing justice. Moreover, there are at least two obvious problems with the argument.

First, one-third of the negotiated settlements of identity civil wars that last for five years "stick." We can not simply say that such settlements do not work, although we need to know more about why some work better than others and, in particular, why their record in identity wars seems so poor.

Second, military victory by definition is the ability to threaten the enemy's civilian population with impunity (Schelling 1966). The winning side can punish its enemies by genocide rather than war. Presumably this is much less likely after a negotiated settlement, where both sides keep some of their power. Barbara Harff reports cases of genocide *after* identity civil wars (mass murder before or during the war already being included in the earlier indicator of intensity) in 19% of the military victories and none of the negotiated settlements (see Table 3). The difference is not significant even by the conventional chi-squared measure, but the numbers are small, and two of the four cells have less than five cases. Noting the risk of overinterpreting small differences, *military victories in identity civil wars may be more likely to be followed by genocide than negotiated settlements*. (Note

TABLE 3

Issue, Method of Termination, and Genocide after Civil Wars

TERMINATION	GENOCIDE (%)	NO GENOCIDE REPORTED (%)	N
Identity civil wars[a]			
Military victories	19	81	27
Negotiated settlement	0	100	10
Political/economic wars[b]			
Military victories	7	93	16
Negotiated settlement	0	100	4

Notes: [a]Pearson's chi-squared = .14; Yates continuity correction = 1.00; likelihood ratio = .06; Fisher Exact Test (one-tailed) = 1.00; 2 of 4 (50%) cells with expected frequency less than 5.
[b]Pearson's chi-squared = .61; Yates continuity correction = 1.00; likelihood ratio = .50; Fisher Exact Test (one-tailed)= 1.00; 3 of 4 (75%) cells with expected frequency less than 5.

that genocide is less likely to follow the termination of nonidentity conflicts in general.)

These data suggest that in political-economic conflict outsiders can reasonably pursue negotiated settlements; they seem about as stable as military victories and are unlikely to be followed by genocide, although the possibility cannot be ruled out. However, in the identity wars that are becoming increasingly common, there may be a dilemma: negotiated settlements are somewhat less likely to "stick" than military victories, but military victories may be more likely to result in genocide or politicide after the war. The key to this puzzle may lie in trying to determine why some negotiated settlements in identity wars "stick" and others do not and why some victories are followed by mass murder while others aren't.

Clearly this is only one of many issues to be explored. How are armed societies disarmed? Under what circumstances do outside interventions (military assistance to one or both sides, mediation, humanitarian aid) help or hinder a settlement? Are lasting solutions more likely before or after total military victory by one side? Does it make a difference whether the underlying issues are ethnic divisions or political differences or whether the goal has been separation from the state or conquest of the state apparatus? Is peace more likely when both sides are strong or when they are divided? Can the central issues be compromised in the initial settlement, or does a draconian solution work best in the long run? Why do some "solutions" last and others collapse? What can we learn from these experiences about the circumstances under which civil societies may be constructed after civil violence? We cannot answer most of these questions now, but it is time to begin examining them, because they are likely to dominate our foreign policy agenda over the next few decades.

APPENDIX: CIVIL WARS FOUGHT AFTER 1945

Table A-1 includes all of the cases as of July 1994 but not all of the variables. I will be glad to send a complete data set to anyone interested. Table A-1 begins on next page.

TABLE A-1

CASE	START & END YEAR[a]	LENGTH[b]	TERM[c]	GEN. BDA[d,k]	DEATH LOG[e]	SOURCE[f]	MORE WAR?[g]	NEXT WAR[h]	ISSUE[i]	STATES[j]
Afghanistan	78	—	1	212	6.1	2	0	2	2	—
Algeria	62/63	1	2	1	3.2	2.	1	—	2	1
Angola	75	—	1	—	5.4	3	0	—	1	—
Argentina	55/55	0	2	—	3.4	2	1	—	2	1
Azerbaijan	88	—	1	—	4.2	3	0	—	1	—
Bangladesh	85/85	0	2	—	5.2	3	2	—	1	1
Bolivia	52/52	0	2	—	3.2	2	1	—	2	1
Bosnia	92	—	1	—	6.1	4	0	—	1	—
Burma	48	—	1	—	5.2	3	0	—	1	—
Burundi	72/72	0	2	111	5.1	2	1	—	1	1
Cambodia	67	—	1	21	6.1	4	0	—	2	—
Chad	80/87	7	3	—	3.7	2.	1	—	1	1
China A	27/49	22	2	211	6.1	2	1	—	2	1
China B (Taiwan)	47/47	0	2	—	3.1	2	3	—	1	1
China C I (Tibet)	50/51	1	2	—	3.2	2	4	5	1	1
China C II	56/59	3	2	221	5.5	2	4	7	1	1
China C III	66/68	3	2	—	—	3	4	20	1	1
China C IV	88/89	1	4	—	—	3	0	—	2	—
Colombia A	48/57	9	3	—	5.3	4	2	—	2	1
Colombia B	78	—	1	—	4.2	2	0	—	2	—
Costa Rica	48/48	0	2	—	3.2	4	1	—	2	1
Croatia	91	—	1	—	4.3	4	0	—	1	—
Cuba	58/59	1	2	—	3.5	2	1	—	2	1
Cyprus I	63/64	1	3	—	—	—	4	10	1	1
Cyprus II	74/74	0	3	—	3.5	2	1	—	1	2
Dom. Republic	65/65	0	3	—	3.3	2	1	—	2	1
El Salvador	79/91	12	4	212	4.8	2	0	—	2	—
Ethiopia	67/91	24	4	212	5.7	1	0	—	1	—
Georgia A (Abkh.)	92	—	1	—	3.1	4	0	—	1	—
Georgia B (Mold.)	92	—	1	—	3.1	4	0	—	1	—
Greece	45/49	4	2	—	5.2	2	1	—	2	1
Guatemala A	54/54	0	2	—	3.1	2	3	—	2	1
Guatemala B	61	—	1	111	6.1	4	0	—	1	—
India A I (Partition)	46/48	2	3	—	5.8	2	4	17	1	2
India A II (Kashmir)	65/65	0	3	—	4.2	4	4	24	1	1
India A III (Kashmir)	89	—	1	—	4.1	3	0	—	1	—
India B (Sikh)	84	—	1	—	4.6	4	0	—	1	—
Indonesia A (Mol.)	50/50	0	2	—	3.5	2	1	—	1	1
Indonesia B (Dar.)	53/53	0	2	—	3.1	2	1	—	1	1
Indonesia C	58/60	2	2	—	4.3	4	3	—	2	1
Indoesia D (Tim.)	75/82	7	2	211	5.2	4	2	—	1	1
Indonesia E (Ach.)	86/86	0	2	—	—	—	1	—	1	1
Iran	78/79	1	2	221	4.9	4	1	—	1	1
Iraq A (Shammar)	59/59	0	2	—	3.2	2	1	—	1	1
Iraq B I (Kurds)	61/75	9	2	221	5.1	2	4	6	1	1
Iraq B II (Kurds)	81	—	1	—	5.1	3	0	—	1	—
Iraq C (Shiites)	91	—	1	—	4.5	3	0	—	1	—
Jordan	70/70	0	2	—	4.1	2	1	—	1	1
Korea A (Army)	48/48	0	2	—	3.1	4	3	—	2	1
Korea B	50/53	3	3	—	6.3	2	1	—	2	2
Laos	59/72	13	2	212	4.3	2	1	—	2	1
Lebanon I	58/58	0	3	—	3.2	2	4	17	1	1
Lebanon II	75/76	1	3	—	5.1	2	4	6	1	1
Lebanon III	82	—	1	—	4.6	2	0	—	1	—
Liberia	89	—	1	—	4.2	3	0	—	1	—
Malaysia	48/59	11	2	212	4.1	2	1	—	1	1
Moldova	92	—	1	—	3.1	4	0	—	1	—
Morocco	75/91	16	4	—	4.2	3	0	—	1	—
Mozambique	81/92	11	4	—	6.1	3	0	—	2	—
Nicaragua A	78/79	1	2	—	4.5	2	3	—	2	1
Nicaragua B	81/89	8	4	—	4.3	2	0	—	2	—
Nigeria A	67/70	3	2	122	6.3	1	1	—	1	1

TABLE A-1 continued

CASE	START & END YEAR[a]	LENGTH[b]	TERM[c]	GEN. BDA[d,k]	DEATH LOG[e]	SOURCE[f]	MORE WAR?[g]	NEXT WAR[h]	ISSUE[i]	STATES[j]
Nigeria B (Muslims)	80/84	4	2	—	3.6	2	1	—	1	1
Pakistan A (Bang.)	71/71	0	2	112	6.1	2	1	—	1	2
Pakistan B (Baluchi)	73/77	4	2	—	3.9	2	1	—	1	1
Palestine	48/48	0	2	—	—	—	1	—	1	2
Paraguay	47/47	0	2	—	3.1	2	1	—	1	1
Peru	80	—	1	—	4.3	4	0	—	2	—
Philippines A	48/50	2	2	—	3.9	4	3	—	2	1
Philippines B (NPA)	72	—	1	—	4.4	2	0	—	2	—
Philippines C (MNLF)	72	—	1	112	4.4	2	0	—	1	—
Rwanda	63/64	1	2	212	5.1	3	1	—	1	1
Somalia	88	—	1	—	5.6	2	0	—	1	—
South Africa	83/91	8	4	—	4.2	3	0	—	1	—
Sri Lanka I	71/71	0	2	—	4.1	2	4	12	1	1
Sri Lanka II	83	—	1	212	4.3	2	0	—	1	—
Sudan I	63/71	8	3	112	5.5	2	4	12	1	1
Sudan II	83	—	1	—	5.6	3	0	—	1	—
Syria	79/82	3	2	212	—	—	1	—	1	1
Tajikistan	92	—	1	—	4.4	3	0	—	1	—
Turkey (Kurds)	84	—	1	—	5.5	4	0	—	1	—
Uganda A (Bagnda)	66/66	0	2	—	3.1	2	1	—	1	1
Uganda B	81/87	6	2	112	5.3	2	1	—	1	1
Vietnam	57/74	17	2	112	6.2	2	1	—	2	1
Yemen (North) A	48/48	0	2	—	3.4	2	3	—	2	1
Yemen (North) B	62/70	8	3	—	4.2	1	1	—	2	1
Yemen (South)	86/87	1	2	—	4.1	2	1	—	2	1
Zaire	60/64	4	2	212	5.1	2	1	—	1	1
Zimbabwe A	72/80	8	3	—	4.1	1	3	—	1	1
Zimbabwe B	83/84	1	3	—	3.2	2	1	—	1	1

Notes: [a]No end year indicates war ongoing as of 1993.
[b]Length of war in years.
[c]Termination:1 = ongoing, 2 = victory, 3 = negotiated settlement, 4 = war ended less than 5 years ago.
[d]Genocide or politicide before, during, and after the war (Gurr 1990; Harff 1993); 1 = reported for the period; 2 = not reported for the period; blank = not reported for the conflict.
[e]Log 10 of reported battle deaths.
[f]Source for battle deaths: 1 = Miall 1992; 2 = Sivard 1992; 3 other; 4 = Sivard 1993.
[g]0= ongoing or ended <5 years; 1 = none; 2 = violence but no war; 3 = war with different sides or issues; 4 = war, same sides and issues.
[h]Years before outbreak of same war.
[i]1 = identity; 2 = political-economic.
[j]Number of states after settlement.
[k]Information on the period after is limited to wars which ended before 1990.

Notes

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1. Clearly 1,000 battle deaths per year is an arbitrary limit, although it is widely used in the literature. My statistical calculations were run separately for the 75 civil wars with reported casualties of 2,000 or more and for the 67 that lasted a year or longer; none of the relationship changed meaningfully as a result.

2. This case was included in earlier versions of this data set. I am indebted to reviewers at the MacArthur Foundation and the United States Institute of Peace for asking me to reconsider its classification.

3. Alternatively, one can argue that the three cases of large-scale violence but no civil war should be included in the *none* category, because the Wagner hypothesis assumes the destruction of the loser's organizational network, which is presumably why the large-scale violence cannot effectively be coordinated into a renewed civil war. The eight examples of different civil wars may also be classified as *no war* for analyzing the Wagner hypothesis, which assumes that the destruction of an existing organization will make it very difficult to resurrect the same civil war, which has apparently happened in these examples. My analysis was rerun twice, once with the large-scale violence cases added and again with these and the different civil wars as well. None of the reported results were materially affected. Copies of these analyses are available upon request.

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ISSUE FLUIDITY ON THE U.S. SUPREME COURT

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In making decisions on the merits, the members of the U.S. Supreme Court are often willing to provide authoritative answers to questions that have not been asked and to disregard issues that the parties have presented. What accounts for these forms of issue fluidity? Analyzing data from the 1988 term of the Court, we find that issue transformation is quite common, occurring in roughly half of the cases on the plenary agenda. We propose models of both issue discovery and issue suppression that, while successful in explaining how the justices select issues, suggest that these two forms of fluidity result from largely different influences.

Scholarly interest in the Supreme Court's agenda has generated considerable insight into the process by which cases are chosen for decisions on the merits. Still, despite our knowledge of how the members of the Court select cases for full consideration, we know precious little about how the justices determine which specific issues to resolve in a case once review is granted. Naturally, the substantive questions that the Court addresses on the merits are apt to be controlled largely by those that are presented by the litigants, but the members of the Court are relatively unconstrained; the justices may use a case as a vehicle for resolving issues not formally presented, or they may elect to ignore issues that were fully briefed and argued. There is no doubt that such issue fluidity is present in a good many cases (see, e.g., Ulmer 1979, 1982). Yet no one has attempted to provide a systematic analysis of this potentially significant variation in judicial choice. To what extent does such issue fluidity occur on the Supreme Court, and what factors account for it?

We shall examine several competing hypotheses regarding the likely causes of issue fluidity on the Supreme Court. With data from the 1988 term of the Court, we examine two different forms of fluidity. Specifically, we model the discovery and suppression of issues for decisions on the merits. Aside from uncovering a fair amount of issue fluidity, our results indicate that a variety of legal and extralegal factors invite and constrain both types of decisions. In each case, our basic design is to explore the difference between the issues that the Supreme Court is *asked* to decide and those that it ultimately *chooses* to decide.

TRANSFORMING THE JUDICIAL AGENDA

Most studies of the Supreme Court's agenda setting focus on the process by which the justices accept and reject appeals and petitions for certiorari (e.g., Caldeira and Wright 1988; Perry 1991; Ulmer 1984). In sifting out all but the most significant cases, the justices obviously narrow the range of issues to which they will devote their scarce attention and resources. That process by itself, however, does not necessarily presage the precise issues to which the Court will speak when it renders an opinion. In other words, each of the cases that the justices agree to decide on the merits will contain issues on which the Court may (or may not) eventually rule. Conversely, a question not formally addressed by the parties may nonetheless be resolved within the context of a specific case. In short, the plenary agenda—which consists of cases—and the issue action agenda—which consists of issues—are not interchangeable, so "the question(s) to which the Court will respond in any given case cannot be known with certainty until the Court's opinion in the case is announced" (Ulmer 1982, 322). Consequently, the process of developing an agenda, as others have rightly noted, must be regarded as a continuous process, one that extends even to decisions on the merits (see, e.g., Caldeira and Wright 1990).

The ability of judges to transform the issues in the cases they decide is, of course, made possible by the malleability inherent in questions of law. In the flow of litigation, for instance, the questions resolved at the trial level are often not those that are addressed on appeal (Marvell 1978; Richardson and Vines 1970). It is therefore scarcely surprising that, as cases make their way through the courts, seemingly narrow, commonplace issues often metamorphose into broader, more consequential concerns. To be sure, lawyers and litigants have a hand in this process, molding the presentation of issues in the hopes of maximizing their likelihood of success. Quite apart from such contributions to the evolution of issues, however, appellate judges—and justices of the Supreme Court in particular—are in large measure free to manipulate the substance of the legal questions before them.

One of the best illustrations of the Supreme Court's facility for modifying the issues with which it contends is evidenced in the transformation of the Court's agenda. Since the 1930s, regularized patterns of change have manifested themselves as the justices have gradually shifted their attention away from economic questions and toward developing doctrines in the area of civil liberties. Indeed, the ebb and flow of the Court's policy priorities can be understood, to a considerable extent, as a function of the changing preferences of the justices themselves (Pacelle 1991). This vacillation in the issue alternatives to which the Court has devoted its energies is visible not only at a broad level of aggregation but within more parochial areas of the Court's docket as well (Caldeira 1981).

Quite clearly, then, there is fluidity on the Court's agenda; it exists at the macro level, both between and within policy domains. What is more, we know a good deal about the various forces that have produced these transformations in the Court's docket. Interestingly enough, however, at the micro level (i.e., at the level of individual cases) our knowledge of issue change is not nearly as well developed. To the extent that the subject of fluidity in judicial choice has been addressed in individual cases, the focus has been on the degree to which there is fluctuation in the votes—as opposed to the issues—before an opinion is announced (Howard 1968). To put it another way, we know how the justices change as a function of the issues in a case, not how the issues in a case change as a function of the justices.

Still, some scholarly signposts do mark the direction for such research. The most significant work on issue transformation of this sort has been largely theoretical. Ulmer (1979) formally introduced the concept and contemplated a number of important variations. In its two most general forms, issue fluidity occurs as either *issue discovery* or *issue suppression.*[1] The former occurs "in any case in which the Court grants full review and then proceeds to discover and decide an issue not raised by the [parties]," while the latter exists "in any case in which review is granted but in which the Court then suppresses and does not decide an issue posed by the [parties]" (Ulmer 1982, 322).

The Court's jurisprudence is replete with both varieties, and each can be readily illustrated. To that end, two prominent cases from the law of search and seizure, *Mapp* v. *Ohio* (1961) and *Schneckloth* v. *Bustamonte* (1973), are often cited as examples of issue fluidity. *Mapp* is perhaps the best known instance of issue discovery. Aside from its obvious constitutional importance, the case is noteworthy in that the application of the exclusionary rule to the states was not an issue formally presented by the litigants. Although originally argued primarily on the question of the constitutionality of a state obscenity statute, the Court's opinion ultimately dealt with the Fourth Amendment, not the First. Here, the justices were apparently persuaded to reach the search-and-seizure issue by the arguments raised in an amicus brief filed by the American Civil Liberties Union (Cortner 1981). Conversely, in *Schneckloth*, the justices ruled that an individual could voluntarily consent to a search, whether or not the person searched was aware of the right to refuse. At the same time, however, a majority of the justices also chose not to address another issue that was clearly raised in the case, namely, whether state prisoners could seek federal habeas corpus review of Fourth Amendment claims (Ulmer 1982, 328–29).

Such decisions—many more of which are easily found—are suggestive of a more general set of behaviors on the high bench. Thus they invite students of the Court to examine more closely this aspect of judicial decision making: How often does issue fluidity occur? Why do the justices bury some issues and unearth others? These questions are especially worthy of attention because they touch directly upon the nature of the judicial role: To what degree are members of the Supreme Court, driven by ideology, willing either to expand or contract issues—to deviate from the questions presented to them by litigants—as a means of promoting their policy preferences? As far as we know, no one has undertaken to answer these questions for a general cross section of the Court's docket.

MEASURING ISSUE FLUIDITY

We hypothesize that legal and extralegal factors affect how the justices select the questions on which to rule in a case, including their decision to enlarge and diminish the range of issues. Accordingly, we begin our analysis by underscoring the importance of distinguishing issues from cases. As much as one might tend to think in terms of the Supreme Court resolving cases, cases simply provide the framework in which issues are addressed. Although this applies with particular force in the Supreme Court, it is no less true of appellate courts more generally. "Issues are the most important information attorneys give an appellate court. . . . Appellate courts create law by deciding issues, not cases," observes one scholar. "Issues determine what legal rules, facts of the case, and social facts may influence the court" (Marvell 1978, 119).

For their part, the justices see the Supreme Court as a forum for resolving significant issues of federal law, not as a court for deciding individual—and often interchangeable—cases: "To say that cases are fungible is not to suggest that an individual case is of no importance, or that differences between cases do not matter. . . . Nevertheless, it is the issue, not the case that is primary" (Perry 1991, 221). Ultimately, the role of the high court, as Chief Justice Vinson suggested, is "to resolve conflicts of opinion on federal questions that have arisen among lower courts, [and] to pass upon questions of wide import" (quoted in Perry 1991, 36; see also Stern, Gressman, and Shapiro 1986, 221–24). Even from a more mechanical perspective, the Court's Rule 10, which provides the only formal guidance on the Court's agenda setting, speaks of the justices deciding "questions," not "cases." It is true, of course, that cases become the currency in which specific issues are exchanged in the world of judges and lawyers, but the cases themselves do little more than provide a kind of legal architecture for the principles of law that they represent.

Being aware of the very real difference between cases and issues is critical to understanding issue fluidity. Many studies of the Court's decision making are justifiably occupied with explaining who wins and loses. From a legal standpoint, however, the outcome of a case is not nearly as significant as the legal basis that undergirds it. So the choice of issues—as opposed to the choice of outcomes—is a matter of much gravity, especially since the justices have considerably more flexibility in justifying their decisions than they do in disposing of them. In most cases, determining which party prevailed is not terribly difficult. Divining *why* from the opinion may be quite another matter: Did the Court discover new issues, suppress existing ones, or simply answer the questions presented?

The justices have continually suggested that they are loathe to manipulate the issues in a case. Rule 21, providing that "only the questions set forth in the petition or fairly included therein will be considered by the Court,"[2] is fairly straightforward in expressing the reluctance of the Court to stray from the record in a case. Moreover, it is not difficult to discern why the members of the Court would hesitate to discover issues not originally raised by the parties. As Justice O'Connor argues, "Prudence . . . dictates awaiting a case in which the issue was fully litigated below, so that we will have the benefit of developed arguments on both sides and lower court opinions squarely addressing the issue" *Yee* v. *Escondido* (1992, 172).[3] Notwithstanding these caveats, the justices are still willing, from time to time, to widen the scope of a case: "Although we do not ordinarily consider questions not specifically passed upon by the lower court, . . . this rule is not inflexible" *Capital Cities Cable* v. *Crisp* (1984, 691). Issue discovery, then, looms inevitably because, "in determining whether to consider issues not raised in a petition, the Court not only has broad discretionary power, but . . . can exercise its discretion so as to permit it to decide the issue it wants to decide" (Stern, Gressman, and Shapiro 1986, 365).[4]

Of course, in the name of judicial restraint, the Supreme Court is occasionally willing to avoid deciding questions that may have been presented on the merits. Justice Brandeis's well-known Ashwander Rules outline a number of circumstances in which the members of the Court may cull the questions in a case: "The Court will not pass upon a constitutional question although properly presented by the record, if there is also present some other ground upon which the case may be disposed of . . . When the validity of an act of the Congress is drawn in question, and even if a serious doubt of constitutionality is raised, it is a cardinal principle that this Court will first ascertain whether a construction of the statute is fairly possible by which the question may be avoided" *Ashwander* v. *Tennessee Valley Authority* (1936, 348).

Naturally, the views on the advisability of issue fluidity are likely to vary from justice to justice and from case to case (see, e.g., Rathjen and Spaeth 1979). Still, the larger lesson that emanates from the Court is that, all things being equal, the justices would prefer to stay closer to the issues presented, rather than deviate significantly from them. To what extent, however, does their avowed aversion to transforming issues belie the actual incidence of issue fluidity?

To answer this question, we collected data on all orally argued cases decided by full opinion during the Supreme Court's 1988 term ($N = 160$). It is certainly true that an analysis of a single term provides but a limited perspective on the Supreme Court. Notwithstanding this limitation, there is no compelling basis to believe that any one term differs radically from the next. Any term is likely reasonably to reflect more general patterns of behavior that endure over time. In each case, we measured issue fluidity by comparing the questions presented by the parties in their briefs against the issues outlined in the syllabus of each case in the *U.S. Reports*.[5] Following Ulmer's (1982) lead, we coded issue discovery as present in any case in which a majority opinion ruled on one or more issues that were clearly not discussed in the "Questions Presented" section of the briefs on the merits of either the petitioner or the respondent. Likewise, we regarded issue suppression as occurring in any case in which a majority did not rule on a substantive question that had obviously been raised in the parties' briefs.[6] Our coding rules (presented more extensively in the Appendix) dictated that we proceed conservatively, judging fluidity as present only where reasonably obvious from a fair reading of the briefs and the syllabus. By our estimates, fluidity in some form was present in just over half of the sample. Ulmer's (1982) analysis suggests that issue suppression would be the more common form of fluidity, and this is in fact what we find. Specifically, issue discovery was present in a relatively small number of cases (some 11%), while the suppression of issues was considerably more common, occurring in 46% of all cases. Happily, these figures compare quite favorably with the rates at which one state supreme court decided issues not raised and ignored insubstantial ones (Marvell 1978, 119–28). Thus, since our data permit some cautious optimism regarding the adequacy of our measures, we proceed to developing some theoretical perspectives on the decision to discover and to suppress issues at the merits.

We begin by assuming that the justices are ideologically motivated, seeking to maximize their policy objectives (Segal and Spaeth 1993) and that the members of the Court, consequently, may either expand or contract issues to that end. At the same time, however, we suspect that a variety of intervening factors will mediate the impact of the justices' attitudes and exercise significant independent effects on the frequency of issue fluidity. What specific forces would be at work in shaping these decisions?[7]

At the institutional level, we believe several characteristics are fairly conspicuous. A leading candidate among them is the manner in which the high court takes jurisdiction. Cases that arise on appeal afford the justices considerably less flexibility than do those that fall under the Court's certiorari jurisdiction. While scholars have noted that as a practical matter, appeals are generally treated as a discretionary segment of the docket (e.g., Perry 1991, 104–6), they are nonetheless cases that the Court is obligated, by law, to decide on the merits. One of the implications of having this well-defined appellate jurisdiction is that it gives the Court very limited discretion, relative to other cases. The lesson is obvious enough: from a jurisdictional perspective, issues brought on appeal simply must be decided. For that reason, the Court should be less prone to either discover new issues or suppress existing ones for cases arising in this fashion. In contrast, those cases brought via the writ of certiorari should be much more open to both forms of issue transformation.[8]

Still, the justices do reserve to themselves the ability to structure the manner in which cases are decided on the merits. One of the more common ways in which this is accomplished is by consolidating multiple cases that raise more or less comparable issues (Stern, Gressman, and Shapiro 1986, 345). The effect of this practice is to increase efficiency. Cases are merged because the Court is interested in deciding, in the context of a single opinion, an issue or group of issues common to all of them. These shared issues, however, are likely to be but a subset of the larger total number of questions raised in the cases. With a spate of ancillary issues, the justices would probably have little need to discover additional ones. Furthermore, focusing attention upon the primary issues that are shared by the cases should come at the expense of those that are unique to but a single case. Consolidated cases, therefore, should have a higher incidence of issue suppression.

Quite apart from the probable consequences of streamlining the plenary caseload, the number of questions raised in a case is apt to affect how many are answered. For a case with a large number of questions emanating from the record, the justices need not look elsewhere for additional ones. Conversely, in deciding a case raising only a single issue, the Court may find it expeditious to develop another. Thus we expect that the greater the number of questions presented in a case, the lesser the likelihood of the justices engaging in issue discovery. Furthermore, as the number of issues increases, so too should the probability of issue suppression. There is little doubt that parties are far from stingy when presenting issues. A litigant may argue that a case presents a host of important questions, but some—even most—could be profitably ignored. As one former clerk emphatically stated, "it would be a minor miracle if a single case really presented more than one" (Baker 1984, 613). As a general rule, complex decisions produce simple solutions (March and Simon 1958). By logical extension, the more questions presented for resolution in a case, the more likely the justices are to simplify their decision making by jettisoning one or more of them.

These disputes, in virtually every instance, come to the justices after having first been heard in the lower appellate courts. These courts vary widely in their institutional reputations (see, e.g., Caldeira 1983), and members of the Court give careful attention not only to the disposition of the case by the lower court but to the identity of the lower court itself. The justices usually give greater deference, for instance, to those members of the bench whose judgments they most respect (Perry 1991, 125). One way to highlight the degrees of deference to lower courts is in the context of federalism. In the minds of some of the justices, decisions originating from the state courts need to be viewed with a critical eye, while cases in the lower federal courts, in contrast, can be considered as more reliably adjudicated. Reflecting his distrust of the involvement of state policymakers in matters of national concern, for instance, Justice Holmes remarked, "I do not think the United States would come to an end if we lost our power to declare an Act of Congress void. I do think the Union would be imperiled if we could not make that declaration as to the laws of the several states" (1920, 295–96). Generally speaking, the federal bench can boast a much higher caliber of operation than the state judiciaries. Federal judges are chosen more selectively, are more able, are better paid, and receive superior administrative support (Neuborne 1977). Not surprisingly, the high court is reluctant to disturb the decisions made within the circuits (Howard 1981). It strikes us as plausible therefore to suspect that the justices consider the review of federal appeals to require neither the suppression of extraneous issues nor the supplement of additional ones; that is, cases channeled to the justices through the federal courts—where questions of national consequence are considered on a regular basis—should be less subject to change at the hands of the Supreme Court.

Another institutional consideration likely to affect the transformation of issues is the timing of a case in relation to the Court's overall calendar. The Court does not, so far as we know, consciously structure its timetable with an eye toward manipulating the issues on its plenary agenda. Nevertheless, the point in the term at which a case receives formal treatment on the merits could well have implications for the outcome. Although the justices dispense with a tremendous number of petitions at the outset of the term, setting an agenda is an ongoing process, one that continues throughout the session. By convention and formal rules, the procedures for litigating a case in the Supreme Court are highly regularized. So in most instances, the justices, with the help of the clerk of the Court, follow more or less established schedules that govern how and when briefs are filed, arguments are held, and so on. The only step in this process in which the justices have measurable latitude is at the end—deciding when to announce publicly their decision.

Ordinarily, the Court issues decisions in all cases argued during the term by the end of the session in late spring. In effect, then, the justices have considerably more time to develop opinions in the cases argued earlier in the term than they have for those argued later. We predict that the likelihood of issue fluidity varies in direct proportion to the amount of time the justices have available to decide a case: "It is easy in October, when the work of the Court is really just starting up for the term, to imagine that there is an infinite amount of time in which to explore every nuance of a question. . . . But, as I learned long ago, . . .there is not an infinite amount of time. . . . I feel very strongly that I want to keep as current as I possibly can with my work on the Court, in order not to build up that sort of backlog of unfinished work that hangs over one as an incubus throughout the remainder of the term" (Rehnquist 1987, 298–99). Hence, the more time the Court has available to mold and shape its ruling, the more ably it will locate the ductile qualities of a case and draw out new issues. With less time, the justices face the prospect of having to restrict the majority opinion to all but the more central concerns.

In addition to the vagaries of the Supreme Court's calendar, the amount of time that the justices take to render a decision probably bears upon the choice of issues. Some cases present knotty legal dilemmas with which the justices are liable to wrestle for some time, while other, less problematic cases permit prompt settlement. If the members of the Court can quickly coalesce around a proposed resolution of the questions presented, the opinions can be drafted and issued with relative dispatch, whereas deciding whether to deviate from the questions presented in a case takes time. To be sure, there may well be a variety of confounding factors that determine when an opinion is handed down; still, our hypothesis is that the less time the members of the Court need to make a decision, the more likely they are to eschew manipulating the issues and deal only with the legal questions immediately at hand.

One direct gauge of the impact of institutional alliances is the vote in a case. It is often asserted that majorities are maintained by selecting the marginal justice to write the opinion, but there is little empirical support for this contention (Brenner and Spaeth 1988). That by itself, however, does not mean that the opinion writer gives no consideration to the views of individual justices. It has long been recognized that the members of the Supreme Court are sensitive to the sentiments of their brethren when drafting opinions (Murphy 1964), a fact we think should be reflected in the Court's choice of issues. Because opinions tend to be assigned to the more moderate members of the majority, we assert that the breadth of a decision varies with the number of votes supporting it: the smaller the majority, the more limited the scope of the Court's opinion.[9] What this means in terms of our models is that smaller majorities should make issue suppression much more likely to occur and issue discovery much less common.

Whatever the impact of the views of the individual justice, the Supreme Court as an institution still reviews cases from the lower courts with an eye toward reversing those judgments with which it disagrees (Armstrong and Johnson 1982; Baum 1977). Predictably, the high court reverses far more often than it affirms. Bear in mind, however, that righting the lower courts who stray from the fold need not trigger wholesale rejection of their opinions. When reversing, the Court may single out the egregious issue and disregard the others. We think, therefore, that the reversal of decisions made by lower courts should correspond with the suppression of issues.

Affirmances are another matter. Why would the Supreme Court grant review to a case simply to put its imprimatur on the lower court? After all, apart from the obvious concerns over conflict, the importance of the issue, and the like, there really is no need to affirm. Obviously, the easiest, most efficient way to "affirm" is to do nothing, that is, to deny the petition for certiorari. A plausible hypothesis is that the justices are willing to devote their scarce resources to a decision with which they agree because they see it as an attractive vehicle, an opportunity to broaden and extend the law within a general policy area that has already been conveniently crafted to their liking by the court below. Necessarily, such favorable circumstances should smooth the way to transcending the claims over which the litigants contend, allowing the justices to probe additional questions.

Beyond these institutional constraints, the substance of a case probably determines its potential for fluidity. One obvious consideration is whether the justices are called upon to adjudge constitutional questions. Charles Evans Hughes' oft-quoted aphorism is particularly relevant to the subject of issue fluidity. Because "the Constitution is what the judges say it is," the Supreme Court has ample leeway to treble or soften the constitutional aspects of a case. The innate plasticity of such issues is evident in their transformation in the flow of litigation (Lamb 1976), and given that the members of the Court are regularly called to inject meaning into the text, one has every reason to expect a strong relationship between the presence of constitutional cases and the incidence of both types of fluidity in the Supreme Court.

Just as the Constitution is more elastic than federal statutes, so too are some issues more flexible than others (see Levi 1949, 3). Civil liberties issues, for example, strike us as naturally more pliant than economic ones. The logic behind this assumption is that the cases in the former category tend to involve questions that are, on average, presented at a higher level of abstraction than those of the latter. Disputes over equality, fundamental freedoms, and individual rights invoke broad legal principles and thus seem fairly to invite mutation and reconceptualization. In contrast, cases dealing with government regulation of economic activity (patent or antitrust controversies, questions of labor law, and disputes over transportation and utilities regulation, to name but a few) can be enormously complex and therefore not readily given to manipulation of any kind.

Another conceivable explanation for fluidity aside from the nature of the issues before the Court is the authority upon which the justices rely when disposing of those issues. Among the basic tenets of judicial restraint is the belief that courts should defer to popular decision makers; only sparingly should judges exercise judicial review. Likewise, judges should invariably seek statutory grounds for their opinions, not constitutional rationales (see, e.g., Lamb 1982). Necessarily, then, to the extent that the justices adhere to these maxims, one should expect to see a direct correspondence between the basis of the Court's opinion and the transformation of issues. A court willing to supplant someone else's judgment for its own should be just as open to substituting its own preferred issues in place of those presented. So the probability that the Supreme Court will deviate from the questions in a case—to enter territory that has not been covered or to ignore territory that has—should increase when the Supreme Court exercises judicial review. By implication, the Court should also be less amenable to modifying a case when it grounds its decision in federal law.

In addition, we recognize that the factors that motivate issue fluidity may not be strictly internal to the Court and the content of its caseload. Certain litigants or organized interests might well color the Court's perceptions. Clearly the most influential litigant in the Supreme Court is the United States. In view of his considerable credibility and talents, the solicitor general enjoys great institutional clout (Caplan 1987; Salokar 1992; Segal and Reedy 1988). In the cases in which the solicitor general is involved, therefore, the justices can anticipate a full and fair treatment of the issues. This, in turn, should obviate the need to address questions not covered in the merits briefs or to slight those issues that have been fully presented.

Interests groups, too, are likely candidates for shaping the high court's judgment (Epstein and Kobylka 1992; Lawrence 1990). Groups pursue their policies in the Court through a variety of mechanisms, the most common of which is the filing of amicus curiae briefs (Schlozman and Tierney 1986). Unfortunately, one of the most important questions—whether or not the independent arguments of interest groups as amici curiae are actually embraced by the Court in its opinions—is also one of the most difficult to answer, given that amicus briefs often only duplicate the reasoning of the litigants (see Epstein 1993, 694–99). We make no pretense of resolving that question here. Instead, we simply assert that the presence of amicus briefs increases the chances of the justices seeing a case from a perspective other than that of the parties. In the absence of amici, the information about a case is filtered through two sources, the petitioner and the respondent. This circumstance substantially improves the odds of the Court resolving the case on precisely the grounds on which it was presented. Briefs amicus curiae, even those that replicate the parties, serve as a reminder that the issues should not be viewed solely through the eyes of the litigants. On the other hand, where amici, independent of the parties, actually make novel arguments that advocate deciding a case on broader or narrower grounds, the Court may find them attractive and adopt them.

It is instructive to bear in mind that amici urge the suppression of issues, as well their expansion. Since amicus briefs, in their intended form, are supposed to present information not contained in the parties' briefs, intuition might suggest that amici would lead the Court to discover new issues, not abandon existing ones. Yet amici can—and often do—urge the Court to limit the questions in a case:

> It frequently happens that a party wants a particular argument to be made but is not in a position to make that argument itself. . . . For example, governmental entities often feel compelled, for political reasons, to argue for very broad rulings: eliminate the exclusionary rule entirely, absolute immunity for all governmental employees, etc. But courts, including the Supreme Court, are institutionally conservative and usually prefer to decide cases on narrower grounds if possible. An amicus can suggest those narrower grounds: qualify the exclusionary rule rather than eliminate it, distinguish a prior case rather than overrule it, or dismiss certiorari as improvidently granted, among others. (Ennis 1984, 606–7)

In short, we suspect that amicus briefs are conducive to fluidity of either form. Moreover, even if amici often replicate the parties, we think that their presence still prompts the Court to look outside the parties' briefs for answers to important questions.

Of course, this is not a comprehensive survey of the potential sources of issue fluidity. Nevertheless, within our theoretical framework, we believe that we have covered a wide range of appropriate and promising predictors. We next subject this theoretical orientation to empirical scrutiny.

ANALYSIS

As we have noted, issue fluidity occurs in the Supreme Court in numbers sufficient to merit closer investigation. Why then does the Supreme Court so often explore uncharted questions or disregard existing ones? Our examination takes the form of two explanatory models; one estimates the likelihood of issue discovery, the other, issue suppression. In each model, the dependent variable assumes a value of 1 for the cases in which fluidity did occur and 0 for those in which the Court did not deviate from the questions presented. With our set of explanatory measures (the coding is presented in the Appendix), we estimated the probability of fluidity using probit analysis (Aldrich and Nelson 1984). The results for these models are presented in Table 1.

Both equations offer generally striking performances; they demonstrate overall statistical significance and impressive fits, as measured by the pseudo-R-squared. The model of issue suppression does enjoy greater success. It reduces error by some 73%, while the equation for issue discovery represents a more modest 11% improvement over the null model. Nevertheless, each provides corroborative support for a good many of our expectations. There are a number of forces clearly at work. What particular effects manifest themselves?

One factor whose impact is common to both models is the jurisdiction under which a case is brought to

TABLE 1

Probit Models of Issue Fluidity on the U.S. Supreme Court

VARIABLE	ISSUE DISCOVERY	ISSUE SUPPRESSION
Constant	−3.57** (1.47)	.07 (1.48)
Writ of appeal	−1.48* (.77)	−1.55** (.55)
Consolidated case	−.06 (.37)	.73* (.42)
Number of questions presented	.04 (.10)	1.52*** (.26)
Case from federal appellate court	.43 (.43)	−1.19** (.45)
Days remaining to decide the case	.0031 (.0026)	−.0054* (.0028)
Days taken to decide the case	−.0034 (.0043)	−.0003 (.0045)
Vote of the majority	.29* (.13)	.08 (.13)
Supreme Court reverses lower court	−.89** (.36)	.18 (.37)
Constitutional case	1.29** (.54)	−.36 (.56)
Civil liberties issue	.76* (.45)	−.32 (.41)
Economic issue	−.52 (.68)	−1.90** (.71)
Judicial review	.80 (.73)	−.60 (.74)
Federal statutory interpretation	.83 (.52)	−.55 (.56)
Solicitor general	−.60 (.44)	−.22 (.43)
Number of amicus briefs	.037* (.019)	.084* (.049)
Pseudo-R^2	.49	.89
−2LLR	28.61*	138.07***
Cases with fluidity	18	73
Cases with no fluidity	142	87

Note: Numbers in parentheses are standard errors. $N = 160$.
*$p < .05$.
**$p < .01$.
***$p < .001$.

the Court. Federal law specifies the kinds of appeals that the justices are bound to hear, and despite their frequent dismissals by the justices, this mandatory jurisdiction still translates into less decisional wiggle room. Accordingly, when a case arises via the writ of appeal, the members of the Court are clearly more likely to face it foursquare, without limiting or expanding the basic questions. Not only does the Court feel obligated to hear appeals, it also thinks it necessary to decide the questions they present.

At least one form of fluidity appears to spring from the concentration of both cases and issues. Although the expansion of issues occurs largely independent of such factors, consolidated cases and those raising a variety of substantive issues are likely to be reduced. Of course, condensing cases into a single opinion and pruning away secondary concerns may be advantageous from the perspective of the justices, but from the litigants' standpoint, there is a very reasonable chance that some of the questions important to them will ultimately not be addressed in the Court's ruling. Far more critical, however, is the number of questions the justices are asked to resolve. Indeed, cases that raise numerous issues are especially vulnerable to suppression. The magnitude of this connection is readily illustrated in probabilistic terms: with two questions presented in a case, the chances of the Court ignoring one of them is fairly strong (.34); the probability of issue suppression jumps to .86, however, when a case raises three questions.[10] These estimates comport quite well with the Court's general tendencies; opinions usually address but one or two primary questions. To be sure, there are occasional constitutional kaleidoscopes, such as *Buckley* v. *Valeo* (1976), but they are obviously the exception. When the high court is asked to decide myriad issues on the merits, it separates the wheat from the chaff no less than at the agenda stage.

If the process of deciding which issues truly merit the Court's close attention is made more burdensome by the parties' demanding answers to large numbers of questions, at least some of their work is lightened by the federal appellate bench. Although the rate of issue discovery does not appear to be sensitive to the identity of the lower court, when it comes to suppressing questions, the justices show considerable respect for the issues as framed within the circuits. So whatever opinions the justices may have on an issue, if it was decided by a federal court of appeals, it will probably not be abandoned. When cases are brought to the Supreme Court from other tribunals—courts unaccustomed to dealing with potentially certworthy cases as a matter of course—the justices frequently find it necessary to overlook issues in order to resolve the most important ones.

Time constraints, too, factor into the Court's decision making in different ways. On the one hand, given more time, the justices evince no greater alacrity for expanding the range of questions in a case. The Court is inclined to tease new issues from a case if time permits, but this propensity is rather slight. On the other hand, as the end of the term approaches and the Court strives to clear its plenary agenda, the justices are significantly more willing to overlook one or more issues in an opinion in the name of expediency. In short, there is good reason to believe that the justices' calendar (in particular, the crunch at the end of term) has genuine consequences for the Court's policy outputs. Beyond the time limitations imposed by the term, we find that the number of days the justices need to agree on an opinion has no effect in either equation.

Why do the justices unearth new issues? The results indicate that part of their decision can be attributed to the size of the majority. In marginal cases, all things being equal, moderation is more likely to prevail in the opinion; that is, the justice writing for the Court is not apt to stray from the questions presented. As agreement among the justices increases, however, so does the willingness of the majority to extend the ruling further and resolve other questions as well. We also hypothesized that issue discovery would occur more often when the justices affirmed the lower court, and this is precisely what we find. Thus, among the factors that might prompt the justices to reverse lower courts, the desire to modify issues is not among them. Those cases where the justices affirm the lower court, however, have a significantly higher likelihood of issue discovery. A plausible explanation is that the justices are motivated to affirm, at least in part, by the desire to enter new territory and amplify issues toward which they are already favorably disposed. Given that the Rehnquist Court has affirmed considerably more cases than its predecessors, we think this relationship is especially noteworthy (see Segal and Spaeth 1993, 199–202). In fact, far from simply placing its stamp of approval on the policies of the lower courts, the Court, it would seem, has carefully chosen the fertile ground in which to allow legal issues to bloom. Not only do these results provide some clues to understanding the high affirmance rate of the Rehnquist Court, but they also suggest that in recent years, the policymaking of the Court may have been far more extensive and magnified than one might presume.

Whether there is a metamorphosis of issues in a case also depends upon its subject matter. As expected, constitutional questions revealed their natural elasticity. The breadth of the Constitution's language undoubtedly affords great freedom in recasting legal questions, a fact that the justices seem keen to exploit, at least in the case of discovering new issues. In addition, the Court is quite sensitive to the general issue area of a case. One interesting difference between our two models is that cases involving civil liberties are much more tractable than others (i.e., they raise the probability of issue discovery), while economic issues are decidedly intractable (i.e., they lower the probability of issue suppression). The justices, therefore, have a marked tendency to increase the amplitude of a civil liberties decision before an opinion is announced. In cases involving individual rights and freedoms, the Court is more than willing to flesh out new issues. Economic cases are not given to such expansion, but they are rarely disregarded; if the justices are asked to address a number of economic questions in a case, there is every likelihood that they will answer them all.

There is, of course, much more to the judicial role than can be captured by the measures we employ. The correlates of activism and restrain that we do bring to bear, however, provide virtually no explanatory power in either equation. Declarations of unconstitutionality and the construction of federal statutes have little bearing upon the rate of issue fluidity. If the justices were genuinely concerned with following the dictates of judicial restraint, the reluctance to judge the validity of the actions of executives and legislatures should decrease the frequency with which the Court departs from the issues in a case. Also, when the authority of the Court's decision rests on statutory grounds, one would again anticipate there being less fluidity. We find no support for the assertion that the judicial role enters into the calculus. None of this will be news to adherents of the attitudinal model.

Even if their policy preferences do shape the contours of issue fluidity, the justices still take their cues to a certain extent from the Court's institutional players. The coefficients for the presence of the solicitor general, while not significant, are instructive nonetheless. The solicitor general is the Court's most reliable litigant; and scholars have shown, over and over again, that the justices consistently look to the federal government for faithful representation of the issues at stake in a case, as well as of their relative importance. So it is not at all surprising that as a party, the solicitor general reduces the incidence of fluidity in either form. Organized interests have an apparently prominent role, too. The greater their presence in the form of briefs amicus curiae, the more willing the justices are to search for new issues or abandon existing ones. It is possible that in formulating their opinions, the justices seize upon alternative grounds suggested by amici. After all, "a good idea is a good idea, whether it is contained in an amicus brief or in the brief of a party" (Ennis 1984, 603). Our evidence on this point, however, is only circumstantial.[11] Still, our data do provide fairly persuasive evidence that amici have a hand—in what precise way, we cannot be sure—in reframing the issues before the Court.

Overall, our assessment of these two models does lead us to one important conclusion. It would seem that these two forms of issue fluidity respond to decidedly different influences. With only a couple of exceptions, the reasons why the Supreme Court resolves issues not presented in a case are not the same as those that determine whether issues are cast aside. Issue discovery is more or less a matter of discretion. For example, the justices are more anxious to augment a case with additional questions when they affirm the lower court and when more members support the outcome. Constitutional questions and civil liberties issues likewise provide a more expandable canvas. By comparison, the Court's decision to ignore issues is largely a function of institutional constraints: a less experienced lower court failed to focus attention on the important issues and made it necessary; the case presented too many questions; or there was not enough time remaining in the term to fully develop the opinion. Issue discovery is largely a proactive behavior, while issue suppression is clearly more reactive in nature. In this respect, then, the pattern is quite clear: the Supreme Court discovers issues because it wants to and suppresses issues because it has to.

CONCLUSION

Our purpose here has been to estimate the extent of, and reasons for, issue fluidity on the Supreme Court for decisions on the merits. We have argued that the justices have considerable leeway in manipulating the issues before them, and our data suggest that they frequently take advantage of that latitude. In roughly half of the full-opinion cases, there is a divergence between the questions presented by the parties and the questions ultimately decided by the justices. Issue fluidity, it turns out, occurs quite often. In a significant minority of cases, the members of the Court provide authoritative answers to questions that have not been asked. More commonly, they disregard issues that the parties have presented. The causes of such transformations are largely as we hypothesized. The justices search for cases that permit them to expand their preferences through new issues, while institutional forces often conspire to leave existing issues behind. Such expansion and restriction of questions is not at all inconsequential, because "via the manipulation of issues, the Supreme Court is exercising a role in the American political system for which it is generally not held accountable" (Ulmer 1979, 73). Issue fluidity, therefore, is a topic worthy of more attention.

However convenient it may be to think of the justices as selecting cases, it is in some sense more appropriate to conceive of the agenda process as involving the selection of issues. Viewed in those terms, the suppression of issues actually reflects fairly refined decisions by the members of the Court. That is, it is not simply a matter of placing cases on the plenary agenda; indeed, the justices have fairly discriminating palates, choosing from among the questions presented in those cases. Similarly, the considerable discretion that the justices exercise in building their agenda can be seen with even greater clarity through the lens of issue discovery. Granted, this expansion of issues occurs in a limited number of cases, but given the fungibility of the Court's jurisdictional agenda (see Perry 1991), it is noteworthy that it occurs as often as it does. In sum, our empirical analysis of the transformation of issues leads us to believe that the process of setting an agenda in the Supreme Court is much more complex and continuous than previous research has established. Far from ending with the selection of cases for plenary review, agenda setting is in fact intertwined with the making of substantive decisions.[12] Quite simply, it involves a good deal more than accepting some cases and rejecting others.

The evidence reported here, we think, brings issue fluidity into greater relief, but it does, of course, leave many questions unanswered. For instance, at the broadest level, our results do not speak to the exercise of fluidity at different time periods: Did the Warren and Burger Courts transform case-specific issues in substantially different ways? In light of the profound changes over time in the Court's allocation of agenda space (Pacelle 1991), this is an interesting consideration. To that end, we expect to undertake in the near future a similar analysis of the Warren Court. Another relevant concern is the filtering of issues that takes place at the stage of case selection. Why, in the granting of petitions for review, do the justices sometimes limit review to specific issues or request that the parties present alternative ones (see Ulmer 1982, 331–33)? Furthermore, we can easily imagine models that take account of various other explanatory factors on which we currently lack data. Such specifications could include the impact of circuit conflicts—which might well lead to issue suppression—and the role of landmark decisions—which could well serve as the stage for the expansion of issues (see Ulmer 1982).

Moreover, as revealing as these results may be, they still do not convey a deeper sense of the substantive decisions embodied in issue fluidity. That is, although our models delineate the circumstances under which issue fluidity occurs, they do not untangle the more perplexing question of *which* issues are expanded or contracted: Do the justices broaden an issue because it is narrowly framed? Similarly, will they abandon a question simply because of its boilerplate quality? Obviously, these are relevant considerations, clearly worthy of close attention in future research. In light of the lack of systematic inquiry into this area, however, we think that demonstrating *whether* issue fluidity occurs is itself an important undertaking.

Our analysis does offer some insight into a significant feature of judicial decision making, a set of decisions that the Supreme Court apparently makes with some frequency. As a consequence, we think that our theoretical notions of agenda building in the Court should take issue fluidity into greater account. That the Court's plenary docket is so fluid suggests quite clearly that the setting of an agenda does not end with the granting of certiorari. It is a highly complex and continuous process in which the justices pick and choose issues long after cases are granted review. Understanding how the Supreme Court selects its issues, therefore, is at least as important as knowing how it selects its cases.

APPENDIX

Issue fluidity reflects the extent to which the U.S. Supreme Court, in making decisions on the merits, provides authoritative answers to legal questions that have not been asked (i.e., issue discovery) or disregards legal issues that the parties have presented (i.e., issue suppression). Although perhaps easily grasped in the abstract, measuring the presence or absence of issue fluidity requires some specific coding rules.

Rule 1. Identify the legal questions raised in a case by consulting the "Questions Presented" of both the petitioner's and the respondent's briefs on the merits. Where the respondent fails to state the questions in its brief, we assume (consistent with the Rules of the

Court) that the party agrees with the petitioner's portrayal of the case. If both parties present essentially comparable questions but one brief treats them summarily while the other expands the general issues into more specific subquestions, we count them in their more expanded form.

Since docket numbers are the unit of analysis, where two or more distinct cases are consolidated (i.e., where cases with separate docket numbers are decided with a single opinion), match the individual petitioners with their respective respondents. In other words, determine questions within docket numbers, not case citation. For instance, if two consolidated cases involve the identical respondent (e.g., a particular state), while the petitioners are two distinct parties, identify the questions separately for each case. Thus the number of questions can differ between docket numbers even though they are part of the same case citation. Where multiple parties represent the same side in a case but individually file briefs, consult each set of briefs to determine the number of unique questions.

Rule 2. Identify the questions addressed in the Supreme Court's opinion. We examine the syllabus for each case in the *U.S. Reports*, identifying the Court's specific holding(s) by asking, "To what issue(s), as a matter of law, did the Supreme Court speak, either directly or indirectly?" Thus questions are addressed only in the context of full majority opinions (or sections of an opinion to which a majority of justices subscribe). Although it is certainly true that a minority of justices may discuss a specific legal issue (in a section of what is otherwise a majority opinion, in a concurrence, or even in a dissent), their statements, however specific, do not have binding precedential value.

Rule 3. Identify the presence or absence of issue discovery. In making this judgment, we ask, "Did a majority opinion provide an authoritative ruling on one or more questions not presented in the merits brief of either the petitioner or the respondent?" In other words, do we know more than we would expect to know based upon the questions presented? This can assume one of two forms: (1) the justices may rule on an issue completely unique to a case (i.e., answer a question not presented), or (2) the justices may specifically answer a question presented at a level of generalization greater than is necessary to cover that question (see Ulmer 1979, 70–73; idem 1982, 321–24). Either of these must be true as a matter of law; that is, a judgment of the Court, though it may speak to other questions, is not sufficient. (Note again, we assess whether a majority of justices addressed additional issues, or expanded existing ones, by consulting the case's syllabus. We adopt this approach because the syllabus underscores the central elements of the Court's decisions, as well as the voting alignments supporting them. These summaries are also generally unadorned by distracting dicta, which might otherwise lead one to inflate the incidence of issue expansion.) If both or either appears in a case, then we code issue discovery as having taken place (1), 0 otherwise.

By way of illustration, issue discovery occurred in the case of *Lorance* v. *AT&T* (1989), a case in which a collective bargaining agreement—one that produced changes in a plantwide seniority system—had an adverse effect on certain female employees. Here, the central question posed on the merits was whether "administrative charges filed by female workers under Title VII of the Civil Rights Act of 1964 [are] timely when filed within 300 days of their demotion to lower-paying jobs caused by the operation of a discriminatory seniority system that was designed to advantage males workers over females workers" (*U.S. Supreme Court Records and Briefs* 1988–89, No. 87-1428, Brief for the Petitioner, p. i.). Before answering this question, however, the Court first held that under Title VII of the Civil Rights Act of 1964, "the operation of a seniority system having a disparate impact on men and women is not unlawful unless discriminatory intent is proved" (ibid. p. 900). The Court then went on to hold, in answer to the question presented, that the limitations period for filing charges began when the seniority system was imposed. Whether the Court had to address the issue of establishing discriminatory intent in order to resolve the question presented is, of course, debatable. More importantly, it is also beside the point. As a matter of law, we know the answer to a legal question not presented by the litigants.

Rule 4. Identify the presence or absence of issue suppression. In making this judgment, we ask, "Did a majority opinion fail to provide an authoritative ruling on one or more questions presented in the merits brief of either the petitioner or the respondent?" In other words, do we know less than we would expect to know based upon the questions presented? It is important to note that the Court need not answer all questions specifically and directly. In many instances, the justices may, by answering one question directly, answer a related question by implication (see Stern, Gressman, and Shapiro 1986, 361). What is determinative is whether, upon consulting the questions presented in the merits briefs, one could turn to the Supreme Court's opinion for authoritative answers to each one. If one cannot, then we consider issue suppression to have taken place (coded 1) or not (coded 0).

By our lights, these coding rules lead us to classify issue suppression as having occurred in the case of *Webster* v. *Reproductive Health Services* (1989). This dispute arose over a number of regulations, imposed by the state of Missouri, that generally restricted access to abortion services. Among the seven questions presented for review by the state in its brief on the merits was the following: "Are legislative findings in the preamble to a state abortion bill that 'the life of each human being begins at conception' and that 'unborn children have protectable interest in life, health and well-being' facially unconstitutional?" (*U.S. Supreme Court Records and Briefs*, 1988–89, No. 88-605, Brief for the Appellants, p. i.). By our coding scheme, this question was not answered as a matter of law in a majority opinion of the Court. More precisely, reasoning that "it would be time enough for federal courts to address the meaning of the preamble should it be applied in some concrete way to restrict the activities of such health care professionals," the Court concluded, "This Court *need not pass on the constitutionality of the Missouri statute's preamble*" (p. 490, emphasis added). So although the Court's opinion actually spoke to the issue raised by the state, the Court—quite explicitly, in fact—refused to answer the question presented. In other words, based on the opinion of the Court, we do not know, as a matter of law, the answer to the question presented by the litigants.

We regard these as reasonably reliable coding rules for our dependent variables. Moreover, we have the empirical support of a reliability check, having recruited a graduate student (one previously unfamiliar with our analysis) to code, consistent with these decision rules, both forms of issue fluidity for a random sample of some 20% of the cases. The agreement between coders was very high for both issue discovery (97%) and issue suppression (94%). To make a more formal judgment about the replicability of our measures, we calculated kappa. This statistic of intercoder reliability is especially appropriate for categorical data—even those variables with skewed distributions—and has the added virtue of correcting for chance agreement between coders. With an upper positive bound of 1.0, our measures of kappa for issue discovery and issue suppression, .88 and .87 respectively, are very substantial, suggesting "almost perfect agreement" (Elder, Pavalko, and Clipp 1993, 42–43). Accordingly, we are confident that our measures of issue fluidity are quite reliable.

Below we also report the coding of the variables we utilize in our models of issue discovery and issue suppression:

Writ of appeal
1 if the case was brought under appellate jurisdiction, 0 if under the certiorari jurisdiction;

Consolidated case
1 if the Supreme Court consolidated the case with one or more additional cases and reported them in a single opinion, 0 otherwise;

Number of questions presented
the number of unique questions presented in the parties' briefs on the merits;

Case from federal appellate court
1 if the court whose decision the Supreme Court reviewed was a federal court of appeals, 0 otherwise;

Days remaining to decide the case
the number of calendar days from the date the case was argued to the last day of the term;

Days taken to decide the case
the number of calendar days from the date the case was argued to the date the opinion was announced;

Vote of the majority
the number of justices voting in the majority;

Supreme Court reverses lower court
1 if the petitioning party prevailed on the merits, 0 otherwise;

Constitutional case
1 if the Supreme Court considered a provision of the U.S. Constitution, 0 otherwise;

Civil liberties issue
1 if the case involved a civil liberties issue—i.e., criminal procedure, civil rights, the First Amendment, due process and privacy—0 otherwise;

Economic issue
1 if the case involved an economic issue—i.e., unions and general economic and commercial activity—0 otherwise;

Judicial review
1 if the Supreme Court declared unconstitutional a federal, state, or local act, 0 otherwise;

Federal statutory interpretation
1 if interpretation of a federal statute or regulation was the basis for a part of the Supreme Court's decision, 0 otherwise;

Solicitor general
1 if the federal government was a party to the case, 0 otherwise;

Amicus briefs
the number of amicus briefs filed in the case on the merits.

Notes

We gratefully acknowledge the comments and criticisms of Lawrence Baum, John A. Clark, Samuel Krislov, Charles E. Smith, Jr., Steven S. Smith, and John L. Sullivan.

1. Ulmer (1982) presents a host of different incarnations of issue fluidity. He discusses, for instance, complex combinations of both issue suppression and issue discovery, as well as the fluidity that occurs between the stage of case selection and decisions on the merits. Although we do not deny the importance of these variations, we choose to focus our attention on the two most basic forms.

2. See the *Rules of the Supreme Court of the United States,* reproduced in Stern, Gressman, and Shapiro 1986, 884.

3. This line of reasoning is particularly appealing, given the fungibility of the Court's docket and the justices' expressed need for percolation in the lower courts (see Perry 1991, 220–21, 230–34).

4. In its rules, the Court also makes explicit its right to "consider a plain error not among the questions presented but evident from the record and otherwise within its jurisdiction to decide" (Stern, Gressman, and Shapiro 1986, 896).

5. Using docket numbers as the unit of analysis, we identified these cases in the *United States Supreme Court Judicial Database.* Of course, a more revealing portrait of issue fluidity—one that would permit assessment of which issues were being manipulated and with what substantive effects—could be painted if we were to treat issues (as opposed to cases) as the unit of analysis. The substantial statistical obstacle to this approach, however, is that it would come at the cost correlated errors. (For example, if issues were the unit of analysis, then the error term in predicting whether the Court, say, suppressed an issue in a case would be directly related to the errors for every other issue within that same case.) Necessarily, the data in our sample are at the case level; some 156 cases were decided with signed opinions and 4 contained full opinions that were written per curiam. Data on the questions presented by the parties were taken from *U.S. Supreme Court Records and Briefs* 1988–89.

6. We did, however, depart from Ulmer's conception of fluidity in one important respect by not restricting our analysis to the questions raised by the petitioning party. In the abstract, our different approaches should not matter, given that the justices identify the issues (usually implicitly but sometimes explicitly) when granting a petition for certiorari or noting probable jurisdiction. All things being equal, at the stage of case selection neither party should be making arguments on the merits of an issue. Instead, one party stresses the importance of the issue(s) and the need to reexamine the decision of the lower court, while another party gainsays the significance of the issue(s) and highlights the correctness of the lower court.

Once a case has been slated for a decision on the merits, however, the debate shifts to the substance of the questions involved. Again, the petitioner's brief "should also include points raised by the respondent or appellee as matters of defense, so that the Questions Presented will contain all the issues upon which the Court is called to pass" (Stern, Gressman, and Shapiro 1986, 553–54); but we are not willing to assume that the two parties necessarily agree on the questions. Parties can and do act strategically on the merits, minimizing or enlarging, as the case may be, issues originally presented at the agenda stage (Baker 1984; Springer 1984). In any given case, therefore, there is some likelihood that the two parties will present objectively different questions in their briefs on the merits. In fact, we have such cases in our data set. We think that it is important to consider the issues as presented by both parties. If, for example, the justices elected to decide an issue that was presented by the respondent but absent from the petitioner's brief, the Court would be, under Ulmer's rule, "discovering" an issue that was clearly contained in a merits brief. Our rule is, we think, more in keeping with the spirit of issue fluidity. It is not, in our judgment, a major concern, given that in most cases the parties do not disagree as to the questions presented.

7. The reader should note that in many instances we draw our hypotheses directly from Ulmer's conceptual survey of the likely causes of issue fluidity (see 1982, 335–41). Much of his discussion was appropriately speculative and, by his admission, subject to dispute. We do, in fact, occasionally propose counterhypotheses.

8. For a more recent sample of the Court's docket, this issue would not loom as large, given that Congress in 1988 all but eliminated the range of cases that could be brought on appeal. In the 1988 term, however, a significant minority of cases—24%—were appealed to the Court and decided with oral argument and full opinion.

9. Also, as a very important technical matter, if a majority of the justices cannot agree to a single opinion, then the Court's decision has no precedential value (Segal and Spaeth 1993, 276). This form of issue suppression—where a plurality of justices announces the judgment of the Court—is what Ulmer (1982) refers to as the "disappearing question."

10. These probabilities are calculated by constraining all of the remaining predictors to their mean values (see Aldrich and Nelson 1984).

11. Moreover, another limitation is that we examine only one avenue of interest group activity. Given our knowledge about the credibility of repeat players more generally, it is entirely possible that when interest groups directly sponsor litigation, the incidence of issue fluidity is reduced.

12. Naturally, this implies that issue fluidity is part of a sequence of decisions, a step that occurs later in the process, not earlier. Although we cannot say with certainty when issue fluidity takes place, we believe that logic is on our side. First, we know that in the earliest stages of case selection (i.e., formation of the discuss list) the justices are apt to rely upon very general indicators of a case's importance. In contrast, in making the subsequent decisions to grant or deny review, the Court renders more sophisticated judgments (Caldeira and Wright 1990). So the even more complex question of which issues to suppress or broaden would not likely arise until the Court began rendering decisions on the merits. Second, if decisions about which issues to address were actually made earlier, the justices would more commonly restrict their grant of certiorari to certain questions within a given case. It is true, of course, that the justices do suppress issues when they

grant review (see Ulmer 1982). Any examination of the Court's regular list of orders, however, would demonstrate that this happens only rarely. Finally, some of our significant predictors (e.g., the number of days remaining in the term or the number of amicus briefs) are either wholly unconnected to the process of case selection or not present until the time that the Court actually makes decisions on the merits.

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CONTROVERSIES

THE EMPIRICAL EVIDENCE FOR CITIZEN INFORMATION AND A LOCAL MARKET FOR PUBLIC GOODS

In their 1993 article in this Review, *Paul Teske, Mark Schneider, Michael Mintrom, and Samuel Best sought to establish the microfoundations for a model of a competitive market for public services between local governments in polycentric regions. An important part of their model focused on subgroups of informed citizens, especially recent movers. Theoretical analysis was supplemented by an empirical study of the factors shaping accuracy of Long Island homeowners' information about relative expenditures and tax rates of their school districts. David Lowery, W. E. Lyons and Ruth Hoogland DeHoog criticize the relevance of this empirical evidence, suggesting the atypical nature of education as a service (especially in this site) and challenging the sufficiency of the demonstrated levels of information for generating a competitive market. Teske and his colleagues reply by pointing out the general importance of education throughout American local policymaking and by defending the relevance of their measures and conclusions for their market model.*

COMMENT

Teske, Schneider, Mintrom, and Best (1993) have provided us with a new interpretation of the microlevel foundations of the Tiebout model (1956). Replacing Tiebout's untenable assumptions of perfect information and perfect mobility, they argue that marginal consumers—those entering a jurisdiction who are highly attentive to public goods and services—are sufficiently influential to generate a public goods quasi market and its attendant efficiency advantages. Obviously, this more realistic interpretation has rather profound implications for how we organize the provision of local public goods and services.

Science, however, entails more than presenting a new idea. Evidence also must be presented to establish, however tentatively, its truth status. But the proponents of a theory are often poor judges of such evidence. Being human, we are all too susceptible to any number of inadvertent errors of interpretation. Accordingly, as noted by philosopher of science David Hull, "The self-correction so important to science does not depend on scientists presenting totally unbiased results but on other scientists, with different biases, checking them" (1988, 321). In that spirit we reexamine the evidence presented by Teske and his colleagues in support of their model. The potential importance of their work justifies this examination. It is also justified because it illustrates well the ways—some trivial and some not—in which biases may infect the selection and interpretation of evidence. Thus, we provide a cautionary tale about the very human enterprise of science.

TESTING THE MODEL

To assess the truth status of the model offered by Teske and colleagues, we need estimates of two quantities: (1) how many informed consumers of local government services there are and (2) how many informed consumers are required to produce a market. The model will be supported if the first proportion is greater than or equal to the second. On both sides of the comparison, Teske and colleagues shape the data to support their model.

Estimates of Informed Public Goods Consumers

Numbers of informed consumers are inflated in four ways. First, they examine only education. Critics of Tiebout have long admitted that citizens are highly attentive to education when compared to other local services. For this reason, we excluded education from our analysis of two Kentucky urban areas in which we challenge the empirical foundations of the public choice approach to local public goods provision, thereby conceding that the unusual salience of local education may generate marketlike traits (Lyons, Lowery, and DeHoog 1992). More to the point, to test a model that purports to be generally applicable to local service provision, Teske and colleagues select the best possible case for their analysis.

Even more troubling is the characterization of those who know the relative standing of their district's school spending—whether it is average, above average, or below average relative to others on Long Island—as *informed*. This is equivalent to saying that knowing the relative ordering in average price of BMWs, Fords, and Hondas makes one a fully informed consumer of automobiles. With this measure, Teske and colleagues have set the criterion level for being an informed consumer at an absurdly unchallenging level.

Third, they fail to report a salient fact about Long Island school districts—that they hold annual referenda on spending. As Inman reported in an article they cite, "The fifty-eight Long Island school districts . . . determines its [*sic*] current expenditures per pupil via an annual budget referendum. Fall budgets are submitted by the school board to the voters for approval in the prior spring" (Inman 1978, 54). Few states do this for school spending, and virtually no jurisdictions do so for other services—fire, police, parks, and so on. This highly unusual annual cue must surely raise information levels about education spending. Whatever the merits of this practice, it should be obvious that it makes the case of Long Island school spending highly unusual in a way that seriously undermines the generalizability of the findings to almost all local public choice problems.

Fourth and most seriously, Teske and colleagues profoundly mischaracterize the citizen's choice problem in order to make the data support their theory. Their focus on the single service of education is essential if their later use of findings on the role of marginal consumers of private goods is to have any validity. But the local public goods market is not organized this way. When one "buys" a local school district, one also purchases through location in a municipality police services, fire protection, street lighting, sanitation services, and so on. It is as if when purchasing a sofa, one automatically purchased as well a television set, a refrigerator, and a car. Tiebout fully recognized this by emphasizing *patterns of revenue expenditure* that encompass all of the goods, services, and costs associated with residing in a locality. And Teske and colleagues at one point refer to "the complicated package of local public goods that Tiebout's shoppers purchase along with their residential location" (p. 705). But they failed to recognize its implication that they must do more to support their model than find that 20% of consumers are "informed" about education. They must demonstrate that 20% are "informed" about education, *and* police services, *and* fire protection, *and* sanitation, *and* taxes, *and* all of the other items comprising the local tax and service package.

Estimates of a Market Threshold

The evidence they employ to define a threshold at which the proportion of informed marginal consumers is sufficient to generate a market is just as weak. Four sources are cited, all flawed in one manner or another (p. 709).

The weakest is their reference to Senator Paul Douglas's argument that a competitive market will develop if only 10% of consumers are informed. Wilde and Schwartz are cited as the source of this claim, but Teske and colleagues omit the full quote: "Senator Douglas, the principal legislative supporter of [Truth in Lending Law], asserted, *unfortunately without explanation*, that a competitive market would exist if 'only' 10 percent of the consumers were cost conscious" (Wilde and Schwartz 1979, 543; emphasis ours). What appears to be evidence, then, is simply an unsupported and politically motivated assertion.

Their second source is Inman's (1978) test of the median voter model with data on spending referendums for Long Island school districts. They report that Inman "showed that a highly informed set of voters, making up about 13 percent of the population, was able to make local school budgets respond to their preferences" (1993, 709). But Teske and colleagues fail to note that Inman's central finding is that these 13% are those around the median income level, unlike Teske and colleagues' informed voters, who are at the upper end of the income range. They employ empirical evidence on apples to support a hypothesis about oranges. Indeed, the key implication of Inman's study is that it is the median voters already residing within districts, not recently arriving upper-income consumers, who drive local school budgets on Long Island.

Third, Teske and colleagues cite Thorelli and Engledow (1980) and Claxton, Fry, and Portis (1974), who classify consumers on the basis of information as measured by number of store visits when making major purchases. But are these classes comparable to those used by Teske and colleagues based on knowledge about relative school spending? The high information group in the Claxton, Fry, and Portis study visited an average of 20 furniture stores during which they almost certainly gathered more information than the relative standing of each firm's median price for a couch—the value most comparable to Teske and colleagues' measure. And while the poorest-informed consumers typically visited only two stores, they certainly were able to see more than a single sofa in each. Thus the measures of information in the two studies of private market consumers surely represent something different from what is tapped by Teske and colleagues' measure. Yet their argument hinges on the comparability of the proportions falling into the superficially similar categories of high, medium, and low information. Given the qualitative differences in these categories across the public and private market studies, valid comparisons of proportions are simply implausible.

CONCLUSION

The truth status of Teske and colleagues' model depends on the validity of both sides of the comparison they wish to make, and on both the evidence they cite remains highly questionable. By focusing on an uncommonly salient service, by selecting cases providing an atypical information cue via annual referenda, by using a trivial standard for defining high levels of information, and by ignoring the inherently compound nature of the local public goods market, Teske and colleagues have arranged a test that can tell us almost nothing about most local public choice problems. If anything in their results is surprising, it is that in so stylized a setting only 19% of the respondents could be identified as "highly informed." And by failing to provide credible evidence about how many informed consumers it takes to produce a competitive market, we cannot know if this 19% is sufficient to generate a public goods quasi market.

While this does not mean that their model is invalid, it does imply that it remains untested. Perhaps more importantly, our analysis indicates that we need to be more cautious in evaluating how proponents of a theory confront it with data. Being human, we are all too inclined to interpret evidence selectively when motivated to do so, however inadvertently. And as David Hull has cautioned, "Any scientist who is incapable of wriggling a bit will never succeed in science, but there are also limits to the wriggling. If it becomes too pervasive, the scientist ceases to be a scientist" (1988, 281). In our view, Teske and his colleagues skate too close to those limits.

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RESPONSE

Scientific progress is often contentious. As a recent winner of the prestigious John Bates Clark Award in economics, Paul Krugman, observed: "In general, if people in a field have bogged down on questions that seem very hard, it is a good idea to ask whether they are really working on the right questions. Often some other question is not only easier to answer but actually more interesting! (One drawback to this trick is that it often gets people angry. An academic who has spent years on a hard problem is rarely grateful when you suggest that his field can be revived by bypassing it)" (1993, 27). Krugman's observation may explain the concluding tone of Lowery, Lyons, and DeHoog's comment, which introduces a highly personalized tone to professional discourse. In this response we address the task of identifying the right questions for future scientific examination of jurisdictional competition.

Understanding our argument requires a more sophisticated view of markets than held by Lowery, Lyons, and DeHoog. By exploring the micro foundations for macro models of the local market for public goods, we address an issue central to the understanding of competitive markets. Their notion that our model is supported only if the number of informed consumers is greater than or equal to the number of informed consumers required to produce a market reduces a complex problem to a trivial and naive standard. There are degrees of market competition and economists have not developed a divining rod that distinguishes "perfectly competitive" markets from "perfectly noncompetitive" ones. If there were such a simple test, antitrust and other complicated market regulation problems would have been resolved long ago.

Lowery, Lyons, and DeHoog admit that macro studies largely show evidence of greater efficiencies in more competitive local government settings, a Tiebout-like finding. But upon finding scant micro evidence in their own work, they simply give up: "This conclusion leaves us with something of a mystery" (Lowery and Lyons 1989, 95). In contrast, we pursue this critical micro-macro link. In so doing, we do not rely narrowly on Tiebout but argue for a fuller theoretical model, including the role of responsive elites and informed business movers, in addition to the informed subset of mobile citizen-consumers that is the focal point of the research reported in our article. (For a fuller development see Schneider and Teske, with Mintrom 1995.)

It is important to note that Lowery, Lyons, and DeHoog do *not* challenge the macro findings of Tiebout-based research or our theoretical development of the mover as a marginal consumer. Indeed, they do *not* challenge any other theoretical element of our work. Essentially, they argue that we have biased the case for finding knowledgeable consumers and that we have not established how many consumers can be shown to make a market competitive. In the next paragraphs, we address these specific concerns. Lowery, Lyons, and DeHoog themselves admit that some of their concerns are "trivial and some not." We will show that this admission is only half right and that their comments illustrate a misunderstanding both of our model and of markets in general.

First, Lowery, Lyons, and DeHoog criticize us for focusing on an "uncommonly salient" issue, education and urge that schools are only part of a complex package of local government. Our simple answer is, yes, we do focus on education. We think our choice is a better foundation for building theories of the local market for public goods than assessing whether or not citizens are aware of which level of government collects their garbage (which was one of Lowery, Lyons, and DeHoog's research questions).

Education, as the most important locally funded public good, makes up the bulk of this market. According to the 1987 Census of Governments, local government spending for education exceeds local spending on streets, water and sewers, parks, libraries, police, fire, sanitation, housing and community development, and transit *combined*. Even for people without children, schools are important because they affect home values. To ignore education as a force shaping the local market for public goods—as Lowery, Lyons, and DeHoog did in their study in Kentucky and as they imply we should do in our work—is short-sighted. We suspect that their omission was motivated more by the fact that with two consolidated school districts as the venue for their research, they could not test the effects of education.

Furthermore, current research suggests that the findings reported by Lowery and Lyons (1989) are anomalous. The political science literature increasingly is focused on movers as a force in the local market for public goods. Recent literature indicates that even low-income people are aware of the quality of services provided by government and that this awareness affects their mobility (Blank 1985; Gramlich and Laren 1984; Peterson and Rom 1989). More generally, Sharp (1986) found that local service and tax reasons were the most prominent ones cited by movers in Kansas City. Percy and Hawkins (1992) found considerable evidence of Tiebout-like behavior in their study of recent movers in the Milwaukee area. In a study of more than eight hundred recent movers around London, John, Dowding, and Biggs (n.d.) found that citizens did enter new communities for reasons related to taxes and services and furthermore that those who said low taxes were important to them were in fact likely to move to a community with lower taxes. The most recent and careful review of Tiebout-related literature summarizes findings from two hundred articles and books: "An impressive array of evidence suggests that the Tiebout family of models holds a number of important truths about urban politics" (Dowding, John, and Biggs 1994). Thus, while we welcome new empirical tests in a variety of settings, our education findings are now part of a growing body of evidence of informed interjurisdictional mobility.

Next, Lowery, Lyons, and DeHoog argue that *our measures of accuracy are too easy*. First, they are factually wrong in stating that we tested only one dimension of local finance: our respondents had to be right about *both* their relative tax rate *and* their school expenditures per capita. These are of course related but not completely so, since districts with large contributors to the local tax base, such as industrial parks or shopping malls, can have low tax rates but high expenditures. Second, if the level of accuracy we used is "absurdly unchallenging," then why were 80% of our respondents wrong? Moreover, numerous studies show that most Americans know very little about politics in general. If they can't name the vice-president, do we really expect them to rank their school district with pinpoint accuracy?

But far more important theoretically, competitive markets do not require the precise levels of accuracy that Lowery, Lyons, and DeHoog imply consumers must have. Similar to work in political science that shows low levels of citizen information, marketing studies of consumer knowledge of product price have found that most consumers lack information that "conventional wisdom" would tell us is required for making markets function competitively. Not all consumers need full information about products to make markets work. The "perfect information" assumption advocated by Lowery, Lyons, and DeHoog is a perfect example of scholars mistaking for reality a simplifying assumption made only for the purposes of model construction. Confusing reality and models can lead to a misunderstanding of both.

Lowery, Lyons, and DeHoog argue that by choosing our research "backyard," we have biased the results, because *New York State has annual school budget votes, which is unusual*. They are technically correct when they claim that few states require votes on school budgets and that such referendums are rare for other local government services. However, to argue that this provides an "unusual annual cue" that damages the generalizability of our findings is wrong. Although only New York, New Jersey, New Hampshire, and Vermont require annual voter approval of the school budgets, in many more states voters are required to approve tax increases. Furthermore, the politicized nature of school board elections and school administration all over America (see Chubb and Moe 1990) ensures that considerable information is available to citizens.

More generally, while the great diversity in American local government structure and practices provides scholars with research opportunities, it also means that *any* area of the country analyzed will be somewhat unique. In fact, Percy and Hawkins explicitly take Lowery, Lyons, and DeHoog to task for their choice of Kentucky, because counties there are far more important than in other regions, and this clearly biases their findings (1992, 1153).

Finally, even if school referenda did provide more information to our respondents than most others in the country receive, this critique is completely off target because our study is focused on the knowledge of citizens who have *recently moved into their district*, most of whom have not been exposed to that jurisdiction's school referenda. If anything, this should lead to longer-term residents (the average consumers) being far better informed from these school referenda than are recent movers (the marginal consumers). In fact, since upper-income movers take advantage of the many low-cost information sources available to them, such as real estate brokers, they hold more accurate knowledge.

Lowery, Lyons, and DeHoog further suggest that the marketing literature on consumer information in competitive private markets *is not comparable to our findings, because private market consumers surely gather more information when they shop*. They provide no evidence for that assertion, because there is none. Every study in the marketing literature that we know of finds a segmentation of knowledge, with only a core of highly informed consumers, who often act as market mavens or opinion leaders. If an informed minority can drive a competitive market for peanut butter or refrigerators, surely such a group can provide competitive pressure when they consider the taxes and services related to the purchase of a house, the largest investment most people ever make.

Our article does not argue that informed entry by some citizens alone drives competitive markets for local public goods. We do suggest that the focus on informed entry is a useful theoretical approach that links seemingly disparate findings and that has empirical evidence in its favor. In combination with business movers, for whom the incentives to make informed choices are great, citizen political voice, which we have shown is linked to mobility, and the strategic behavior of governmental elites, a solid foundation can be laid to support the macro finding that local governments in polycentric metropolitan areas are pressured toward efficiency.

We do not care if Tiebout's abstract model is "saved." In our view, the interesting question is, Given that so many citizens and consumers fail correctly to answer "absurdly unchallenging" questions about the prices of various goods and services, how is it that so many markets work competitively? This question will become more important as privatization, vouchers, and other government reforms are enacted, because they all encourage greater citizen choice in quasi markets that may or may not cross jurisdictional boundaries.

For political scientists, there are new and important questions to address: How do institutional structures affect citizen incentives to gather information? How do different kinds of citizens structure interactions to obtain information they want? How accurate is the information they obtain? How do they make use of it? At present, outside of some work related to voting behavior (Delli Carpini and Keeter 1995; Huckfeldt and Sprague 1991; Lupia 1992, 1994; Popkin 1991; Williams 1994), little work in our discipline addresses these questions. With the support of the National Science Foundation, we are presently examining these questions in the context of public school choice.

There is much we can learn from a diverse group of scholars who are extending microeconomic models of decision making to incorporate aspects of social interactions. Examples include studies of the use of social networks to acquire information on employment opportunities (Montgomery 1994), herd behavior in financial markets (Banerjee 1992), the role of gossip in organizations (Banerjee 1993; March and Sevon 1988), information cascades (Bikhchandani, Hirshleifer, and Welch 1992; Lohmann 1994), and the decision processes employed by corporate buyers (Bunn 1993). Political scientists interested in information problems must look across disciplinary boundaries as they develop their research questions and strategies. At times, this approach will anger to those who choose to remain on familiar turf, leading them to substitute emotional reactions for reasoned argument.

We have provided an important new idea and tested it. As social scientists, we look forward to better-reasoned critiques and more challenging refinements to this work.

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THE ENACTMENT OF MOTHERS' PENSIONS: CIVIC MOBILIZATION AND AGENDA SETTING OR BENEFITS OF THE BALLOT?

In this Review in 1993, Skocpol, Howard, Lehmann, and Abend-Wein analyzed the rapid enactment of mothers' pension laws in the American states in the 1910s. They concluded that the widespread federations of women's voluntary groups exerted a powerful influence on these enactments even before most American women had the right to vote. Sparks and Walniuk challenge these conclusions, noting that all 10 equal-suffrage states are among the 29 that passed mothers' pensions before 1916, and presenting new measures of suffrage endorsement and suffrage pressures in regression analyses suggesting that women's votes—actual and potential—played a major role in leading some states to adopt mothers' pensions earlier than their normal patterns of legislative change would predict. In response, Skocpol defends the 1993 conclusions. She adduces that the aggregate pattern of enactment of mothers' pensions corresponds much more closely to endorsements by women's groups than to suffrage timing and puts forth reasons to doubt the validity and significance of Sparks and Walniuk's tests.

COMMENT

Since the institution of woman suffrage at the federal level in 1920, women have, for the most part, failed to vote as a cohesive bloc. Prior to passage of the Nineteenth Amendment, however, there is considerable evidence that women from a wide range of backgrounds overcame their differences and joined together to advocate legislation favorable to women and children at the state and, occasionally, at the national level. The campaign for mothers' pensions provides a good example of this type of activism. As Skocpol, Howard, Lehmann, and Abend-Wein (1993) report, voluntary women's organizations (most notably the General Federation of Women's Clubs [GFWC] with over one million members) played an active role in securing mothers' pensions in most of the 40 states that enacted these laws between 1911 and 1920. Skocpol and her associates argue that a key element in women's activism was the desire to create a "maternalist welfare state," which pulled women into action against political corruption and the lack of governmental concern for children's and women's welfare. Working for reforms such as improved sanitation, stricter laws on the purity of food and milk, and the abolishment of child labor was seen as a natural extension of women's traditional role as homemaker, since these issues directly affected women's ability to provide healthy and nurturing homes for their children.

The authors also assert that it was women's position as outsiders in the political structure that gave them the leverage to lobby successfully for these reforms. Since they were not participants in the dirty world of politics, women could rise above the typical constraints of partisanship and economic interests and argue for reforms on the basis of moral principles and the future of the nation's children. Thus, women were able to appeal to state legislatures in the name of informed motherhood and exert a strong influence in favor of reform legislation "even before most American women had the right to vote" (1993, 686). That women had this level of political influence prior to suffrage adds weight to the assertion that they may have gained very little when they gained the right to vote (see also O'Neill 1969a; Skocpol 1992). Given the increased division on many social and welfare issues in women's organizations such as the GFWC and the concurrent decline in social and political activism among women in general during the 1920s, it could even be argued that women were more successful in achieving their goals through the indirect methods of lobbying by women's groups than through the direct method of using their power at the ballot box.

The lobbying efforts of women who were members and officers of the GFWC and its constituent clubs were, without doubt, important to the passage of mothers' pensions. Our evidence suggests, however, that this explanation is too simple, because it fails to take into account the pressure that was being exerted on state legislatures by the suffrage movement during the decade preceeding the Nineteenth Amendment. The question that must be asked is whether women's organizations (which had only recently become politically active) could have had the same level of influence at any other historical juncture.[1] In other words, were legislators responding to the proposals of these groups due to women's position outside the political system or due to the probability that they would soon be part of that system? As we will show, the enactment of mothers' pensions was strongly influenced by the suffrage campaigns that were in progress throughout most of the nation—with considerable assistance in many states from the local clubs and state federations that were members of the GFWC. Legislators were more amenable to the legislation that women favored, including mothers' pensions, when they were faced with either the reality of female voters or the strong possibility of female voters in future elections.

MOTHERS' PENSIONS AND WOMEN'S ORGANIZATIONS

Compared to other social welfare policies, the spread of mothers' pensions in the 1910s was rapid and follows a pattern not predicted by standard political science theories on legislative change (Gray 1973; Walker 1969). Following up on her book, *Protecting Soldiers and Mothers* (Skocpol 1992), Skocpol and her associates test a variety of hypotheses concerning the effects of "economic conditions, culture and ideology, electoral politics, governmental institutions and prior public policies, and the role of business, labor and women's voluntary groups on the priority of state enactments" (1993, 686). She concludes that much of the credit for the quick adoption of this program can be attributed to the influence of women's voluntary groups that were located throughout the nation. The strong organization and rapid mobilization of women's clubs on this issue helped to "rapidly promote a nearly nationwide debate about the moral desirability of providing public aid to worthy widowed mothers and their children" (p. 697). Using Walker's (1969, 1971) model (which cites wealth, industrialization, and urbanization as key factors in legislative innovation), Skocpol and her associates identify 14 states that enacted mothers' pensions more rapidly than the model would suggest. They incorporate a measure of WOMEN'S GROUP ENDORSEMENTS to explain why this legislation was adopted so rapidly, even by states usually slow to respond to new policies. The two other variables that remain significant in the final model they presented are STATE EXPENDITURES ON PUBLIC EDUCATION and PERCENT OF WORKFORCE FEMALE.

Skocpol and her associates present ample evidence that organizations such as the GFWC played a substantial role in promoting this policy. In all but 5 of the 28 states that adopted this legislation between 1911 and 1916, women's groups within the state endorsed the policy prior to its passage and often actively lobbied on its behalf (see Appendix).[2] The key question, however, is whether it was the lobbying efforts of these women or the power of their votes—actual and potential—that was the critical factor in bringing this legislation to the front of the agenda in each of these unusually innovative states.

Although it is true, as Skocpol (1992) notes, that those in favor of mothers' pensions often tried to keep the issue separate from the suffrage question, this was difficult to do. Most of the women's organizations who were involved in the reform efforts (e.g., the Women's Christian Temperance Union, the National Consumer's League, and the GFWC) also endorsed suffrage, and many suffrage organizations were also involved in efforts to push reform bills—including mothers' pensions—through the legislature. Suffrage periodicals such as the *Woman's Journal* and *Woman Citizen* were filled with articles and editorials stressing the need for laws better to protect women and children, including minimum wage and hours laws, equal guardianship of children, and mothers' pensions. These publications also kept track of the status of reform bills in both suffrage and nonsuffrage states and frequently remarked on how much easier it was for women to obtain "good laws" when they had access to the ballot.[3]

The GFWC, the organization to which Skocpol attributes most of the effort for mothers' pensions, did not officially endorse women's suffrage until its national convention in 1914, but most leaders and many members supported the cause at the state and local level much earlier (see Appendix).[4] Furthermore, there is a strong pattern of dual membership among the leaders of suffrage organizations and GFWC affiliates. Many of the women who were most actively involved in the suffrage movement held leadership positions in these clubs. Fully 94% of suffrage leaders in the National American Women's Suffrage Association (NAWSA) and 85% of suffrage leaders in the more radical National Women's Party were members of clubs affiliated with the GFWC (Benson 1977, 190–209).

Skocpol asserts that suffrage was a secondary goal for most women's organizations, necessary primarily as a means of creating a governmental structure that would be more amenable to women's maternal concerns (1992, 19). This may be a reasonably accurate depiction of rank-and-file members of the local affiliates of these organizations and some of the leaders as well. Many others, however, were convinced that suffrage was an issue of justice; for them, expedience arguments were simply the most effective way of getting more women involved in the fight.[5] Club leaders—most of whom were also suffragists—realized that one way to motivate women who were indifferent to the campaign for suffrage (or who felt that suffrage was unladylike) was to appeal to their maternal instincts. Showing the connections between the health and welfare of the community and that of individual families helped to encourage women and women's organizations to take a more active stand on issues that affected them directly or indirectly. Child labor laws, protective labor laws for women, purity of food, and mothers' pensions were just a few of the issues on behalf of which women were urged to write or visit their legislators. By setting up civics committees to educate members on these and other issues of the era and by urging other state and local clubs to do the same, club leaders were able to shape the political beliefs of large groups of women who had previously been uninvolved (and, largely, uninterested) in politics. Furthermore, setting up legislative action committees allowed a few club activists to lobby for reforms, including, in many cases, suffrage. The fact that they represented a much larger organization of women undoubtedly gave increased weight to the requests they made of state legislators.

Whether women sought the vote for reasons of justice or reasons of expediency is largely irrelevant, however, to the consideration of why legislators were particularly responsive to the policies endorsed by women's groups. Regardless of why women wanted the vote, legislators were facing strong indications that most women did want it and were willing to work to get it. For this reason, the legislative activities of "organized womanhood" in this era cannot be fully understood outside of the quest for suffrage. In the states where women were allowed to vote before the Federal Amendment, it is apparent that women's activism could have had a direct impact on legislative decisions simply through the expected power of their votes. We assert that the influence was also considerable in nonsuffrage states, given the strength of the suffrage movement in most of the nation and the high degree of uncertainty surrounding the possibility of women voting in future elections.

EFFECT OF SUFFRAGE CAMPAIGNS ON THE SKOCPOL MODEL

Much effort was being expended by women's organizations and others on behalf of suffrage, and legislatures were responding—albeit to varying degrees. The year 1913 provides an interesting example. Of the 18 states that approved mothers' pensions in that year, six were suffrage states. Eight of the remaining 12 states had passed legislation in both Houses within recent sessions to submit the suffrage issue to popular referendum.[6] A ninth state, Nebraska, also held a suffrage referendum in 1914, but as a result of a petitioning campaign instead of legislative proposal. In each of the three remaining states (Massachusetts, Minnesota, and New Hampshire), the governors had endorsed suffrage, and in Massachusetts and New Hampshire, at least one major party had endorsed it. In Minnesota, an amendment to submit the question to the voters passed the House but failed to pass in the Senate by two votes in 1911 and three votes in 1913.

In all of these states, it would seem that legislators had cause to believe that women might be voting in the foreseeable future. This belief would have become even stronger as more states granted suffrage, because this increased the likelihood of a successful federal amendment. One way for legislators to gain the support of this block of potential female voters was to endorse suffrage. In some cases, however, appeasing the potential voters could risk losing the support of actual voters. Another, less costly, option was to support the types of legislation that women wanted to see enacted.

There is evidence that legislators were indeed considering the possibility of suffrage when making their decisions. Like contemporary members of Congress, these individuals were risk-averse in situations that could affect their chances for reelection. Comprehensive accounts of suffrage campaigns for each state are given in volume 6 of *The History of Woman Suffrage* (Harper 1922) and in issues of the *Woman's Journal*. These indicate that state legislatures were being bombarded with suffrage bills. In some cases, suffragists were actively campaigning against legislators who were staunch opponents of granting votes to women, occasionally bringing about their defeat. For example, the suffrage organizations in Massachussetts responded to the repeated rejection of their suffrage bills by compiling and publicizing the voting records of antisuffrage legislators during 1912–13 on suffrage and other "liberal measures." One of the targeted legislators was defeated in 1912, and an additional five were ousted in 1913 (ibid., 295–97). Other states adopted similar tactics. By 1913, virtually every legislature (including many in the South that had previously seen little or no activity for suffrage) faced an organized and persistent lobby of women who wanted to shift the focus of the government toward women and children and who wanted the vote to help them accomplish the necessary changes.

The focused action of women who lobbied the legislature for both reforms and suffrage led to a general perception among legislators that women might vote as a bloc. (A major reason that the suffrage movement encountered such resistance from the liquor industry was the prevalent belief that women's bloc vote would usher in prohibition.) The fear of a bloc vote is cited by Flexner as a key reason why the issues that women's groups were promoting were most effective shortly after winning suffrage and then declined throughout the latter half of the 1920s as it became increasingly clear that women's votes were not following a gender-dictated pattern (1975, 338). Anticipation of a bloc vote may also be a reason why the issues that women endorsed were especially effective in the period just prior to suffrage, because legislators had to consider the possibility that their electorate could be doubled in upcoming elections by the addition of women.

Skocpol and her associates, however, assert that women's votes were *not* a significant factor in the early passage of mother's pensions. This conclusion may be attributable to the way that the variable was measured in that study—simply according to whether a state granted suffrage prior to the enactment of the federal amendment in 1920. It is hardly surprising that suffrage failed to be a significant variable under this condition, because many states that did not grant women's votes prior to 1920 found reform legislation, including provisions for mother's pensions, to be politically feasible. Not only did such legislation win them a certain amount of favor in the eyes of the women of their state (many of whom they were alienating by denying suffrage), it also received little opposition from the liquor interests and cost the taxpayers very little. This was certainly true in the case of mothers' pensions, because the cost to the state was usually less than that of maintaining the current system, which consigned children of impoverished mothers to orphanages. Mothers' pensions also appealed to the more traditional legislators who advocated restriction of women's roles to the private sphere, because the policy aimed to keep mothers at home with their school-aged children.

We conducted ordinary least squares regressions to see how additional variables affected the final model

TABLE 1

Models for Explaining Timing of Mothers' Pensions Enactments

VARIABLE	SKOCPOL MODEL B[a]	SKOCPOL MODEL BETA[b]	SKOCPOL MODEL WITH SUFFRAGE VARIABLES B[a]	SKOCPOL MODEL WITH SUFFRAGE VARIABLES BETA[b]
Constant	2.790 (.614)	—	2.395 (.414)	—
State expenditures on public schools	.094** (.000)	.362	.020 (.019)	.077
Percent work force female (−)	.039* (.014)	.233	.022* (.010)	.130
Women's group endorsements	.515** (.086)	.485	.123 (.077)	.116
Suffrage at time of mothers' pensions	—	—	.782** (.124)	.660
Suffrage campaigns	—	—	.817** (.109)	.803
R^2	.814		.920	
Adj. R^2	.801		.911	
SE	.790		.529	

Note: N = 48. All estimates were derived using the OLS routine in SYSTAT Version 5.2.1. For unknown reasons, there are discrepancies between the coefficients presented here and those published in Skocpol et al. 1993. These are generally minor, except for the variable *state expenditures on public schools*, which is larger by a factor of 100, possibly due to a scaling error in the Skocpol et al. study. The adjusted R^2 and standard error reported here are, however, almost identical to those cited in the original (adjusted R^2 in orginal = .794, standard error = .80. Standard errors are in parentheses.

[a] Unstandardized coefficients.

*$p < .05$.

**$p < .01$.

[b] Standardized coefficients.

presented by Skocpol and her associates. Maintaining both their dependent variable and independent variables for the time being, we analyze two additional variables. The first, SUFFRAGE AT TIME OF MOTHERS' PENSIONS, addresses the question of whether women were already voting in the state when mothers' pensions passed. The second new variable, SUFFRAGE CAMPAIGNS, asks whether a statewide suffrage campaign was going on when mothers' pensions legislation passed.[7] One way to measure this would be to look at the status of suffrage legislation in each state. This is, however, very difficult to standardize, because states varied widely on many of the indicators: the number of legislatures that a bill had to pass prior to inclusion on the ballot, how often the legislature was in session, the strength of antiprohibition lobbies, and in several cases, whether suffrage questions could even be addressed by referendum.

An alternative way to determine the extent to which decisions on mothers' pensions were influenced by the impending threat of suffrage is to measure the amount of pressure being aimed at the legislature by the suffrage organizations and their allies. This is the method that we used to construct our SUFFRAGE CAMPAIGNS variable. Consulting year-by-year descriptions of the status of suffrage campaigns, each state was coded for whether or not its legislature faced a significant statewide campaign for suffrage during the two-year period prior to the passage of mothers' pensions. (See Appendix and Table A-1 for information on sources.) Suffrage activity was ongoing throughout the nation, with the exception of a few southern states, but some of these were the sort of low-level efforts where a bill requesting suffrage for women was submitted to the legislature year after year, only to be ignored or voted down by a substantial margin. This had been occuring for decades in many states with little or no effect, because the suffrage organizations lacked the support of political parties and powerful state leaders. Therefore, only those states that met one or more of the following requirements were counted as receiving significant suffrage pressure: suffrage referendum approved by the legislature, major political party endorsement, endorsement of the governor, employment of full-time suffrage lobbyists or state organizers, or financial assistance from NAWSA.[8] Since NAWSA's funds were limited, it tended only to give financial support and contribute workers to those states perceived as likely wins or, later, to states that might be convinced to ratify the federal suffrage amendment. Finally, states that passed mothers' pensions in 1918 and 1919 are coded as receiving significant suffrage pressure, given the probability that the matter would be decided by the pending federal amendment and given the nationwide campaign for ratification that was funded by NAWSA and other suffrage organizations.

Both of the new variables display considerable explanatory power (see Table 1). A test of the difference between this model and the Skocpol model shows that the revised model is the stronger of the two ($p < .001$). With the addition of our suffrage variables, WOMEN'S GROUP ENDORSEMENTS is no longer significant ($p = .12$). This suggests that the rejection of suffrage variables in the final Skocpol model was premature.

AN ALTERNATIVE MODEL

In order better to understand the reasons for the "wildfire spread" of mothers' pensions, we have developed a model that takes into account the political environment in which these policies were enacted. Unfortunately, the dependent variable used in the Skocpol model fails to control for state variations, because it is measured by grouping the 48 states on a seven-part ordinal scale, simply according to when the state passed legislation providing for mothers' pensions.[9] The authors state that this measure was chosen because "a single chronological year of time simply does not have the same meaning throughout the 1911–35 period. During the 1910s, events having to do with social reforms moved very rapidly, and most state legislatures met on alternate years. It made a difference whether a state enacted mothers' pensions in (or before) 1913 (during the first major wave of enactments) or in the successive waves that peaked in 1915 and 1917" (1993, 687). While this is a valid point, the ordinal measure fails to get at the central issue, namely, why some states passed this legislation not only sooner than others but sooner than their *own* patterns of adopting new programs would predict. Wealthier, industrialized, and urban states generally take the lead when new programs spread across the nation (Walker 1971, 360). When the states are divided according to an ordinal scale like this one, however, the adoption of mothers' pensions in 1913 in a state such as Idaho is equivalent to the same act in wealthier, more industrialized, and more urban states such as Massachusetts or New Jersey.

Walker's model of legislative innovation provides a useful way of examining the differences between the average pattern of diffusion for these 48 states and the spread of mothers' pensions.[10] Indeed, Skocpol and her associates use this model to identify the 14 "innovative" states that adopted pensions much earlier than would be expected. We have devised an "innovation score" on the issue of mothers' pensions for each state by first calculating the number of years between the time that the first state (Illinois, in 1911) and the last states (Georgia and South Carolina, in 1935) adopted this legislation. Next, each state was assigned a number based on the proportion of time between the first enactment and its own. Illinois was coded as 1.000 (most innovative), with Georgia and South Carolina receiving a score of 0. We can then take a composite innovation score (coded in the same fashion) for each of the 48 states (Walker 1971, 358) and compute the difference between innovation in the particular case of mothers' pensions and average innovation for that state. This gives us a score that not only considers how early a state passed the legislation but also factors in whether early adoption of legislation was its normal pattern. This measure, DIFFERENCE IN INNOVATION ON MOTHERS' PENSIONS, is adopted as our dependent variable.

States that were especially innovative on the issue of suffrage seem likely to show similar innovation on other issues that women were promoting, including mothers' pensions. Therefore, we devised a similar innovation score for each state on the issue of suffrage. Wyoming, which entered the Union as a suffrage state in 1890, is coded as 1.000 (most innovative), and those states that did not grant suffrage prior to the Federal Amendment are given a score of 0. Again, the difference between this score and the composite innovation score for each state was computed. This measure, DIFFERENCE IN INNOVATION ON SUFFRAGE, is the first independent variable in the model.

Suffrage organizations had been lobbying on behalf of better laws for women and children in many states since before the turn of the century but had made minimal progress. One reason for this is that their requests (including the request for the vote) were frequently seen by legislators as the demands of a radical fringe that should not be seen as representative of the desires of most women in the state. This became much more difficult, however, once the major (nonsuffrage) women's organizations began to endorse and even lobby for suffrage. An additional factor, therefore, that could have been important in the willingness of legislators to respond to women's concerns is the strength of support for suffrage among the organized women of the state. The next variable in our model, SUFFRAGE ACTIVISM IN WOMEN'S CLUBS, is a dichotomous measure of whether the largest women's organization in each state, the Federation of Women's Clubs, endorsed suffrage prior to the time that mothers' pensions legislation was passed. (See Table A-1 for dates of endorsement and additional information on coding.)

For the sake of comparison, we also include a version of the WOMEN'S GROUP ENDORSEMENT measure used in the Skocpol model. We no longer group this variable according to an ordinal scale, however, because this seems to result in several anomalies, such as counting endorsement by Mississippi women's clubs even though it occurred ten years prior to the securing of mothers' pensions in that state and counting Illinois and New Jersey despite the fact that the clubs endorsed mothers' pensions only after the initial legislation was passed. Including both measures in the model would result in unreliable coefficients, because the original club endorsement variable (WOMEN'S GROUP ENDORSEMENT) and the recoded variable (CLUB ENDORSEMENTS) are highly correlated ($r = .80$).[11] Because the key point in determining whether club activity was important in passing this legislation is whether or not the *current* legislature was subject to lobbying from these groups, we prefer the dichotomized version. We have also recoded our

TABLE 2

Difference in Innovation on Mothers' Pensions

	HYBRID MODEL		FINAL MODEL	
VARIABLE	B[a]	BETA[b]	B[a]	BETA[b]
Constant	.185 (.115)	—	.198 (.114)	—
Difference in innovation on suffrage	.198 (.102)	.231	.209* (.100)	.244
Suffrage activism in women's groups	.140* (.061)	.292	.155** (.058)	.323
Club endorsements	.041 (.048)	.085	—	—
Suffrage pressure on legislature	.132* (.054)	.294	.142** (.058)	.316
State expenditures on public schools	.005 (.003)	.146	.005 (.003)	.150
Percent work force female (−)	.006* (.003)	.256	.005* (.003)	.254
R^2	.717		.712	
Adj. R^2	.676		.678	
SE	.128		.128	

Note: N = 48. All estimates were derived using the OLS routine in SYSTAT Version 5.2.1. *Suffrage pressure on legislature* and *club endorsements* are dichotomous versions of *suffrage campaigns* and *women's group endorsements* included in Table 1. Standard errors are in parentheses.

[a] Unstandardized coefficients.

[b] Standardized coefficients.

*$p < .05$.

*$p < .01$.

variable SUFFRAGE CAMPAIGNS in the same fashion. These two measures (SUFFRAGE PRESSURE ON LEGISLATURE and CLUB ENDORSEMENT) are included as dichotomous variables, based on activity by suffrage groups and women's organizations, respectively, during the two years before mothers' pensions were enacted.[12] We also retain the control variables PERCENT WORKFORCE FEMALE and STATE EXPENDITURES ON SCHOOLS (see Table 2).

Since CLUB ENDORSEMENT does not add significantly to the predictive power of our model, it is dropped from consideration. All of the suffrage variables are significant in our final model ($p < .05$), providing further evidence that the Skocpol model is misspecified, because it fails to take into consideration the political context surrounding the passage of mothers' pensions.

DISCUSSION

Our results suggest that suffrage pressure on the legislature was more important in securing mothers' pensions than endorsement by women's organizations. States that were experiencing strong pressure from suffrage organizations and their allies (SUFFRAGE PRESSURE ON LEGISLATURE) were more likely to enact mothers' pensions, and clubs that were linked to a suffrage movement in their states (SUFFRAGE ACTIVISM IN WOMEN'S CLUBS) were more likely to endorse the legislation. States that were relatively more innovative on suffrage (DIFFERENCE IN INNOVATION ON SUFFRAGE) were also more likely to pass this legislation. While it is undeniable that women's clubs played a crucial role in endorsing and sometimes in lobbying for this program, legislatures seem to have been influenced less by their power as disenfranchised, voluntary agents than by their potential power as voters.

A final point that should be addressed is a question raised by Skocpol (1992). Why did the solidarity that women of different classes and perspectives achieved during the 1900s and 1910s begin to crumble in the 1920s and after? She notes that many women's organizations, including those on which she focuses, "had lost much of their political energy and broad sense of social concern by the mid-1920s" (p. 519). Our conclusions suggest that one explanation can be found in the motivations of the *officers* of these women's groups, who pushed a reformist, social welfare agenda during the first two decades of the century. The leaders of these groups were responsible for organization on a national level and for distributing information on a variety of issues, including reform work, to state affiliates. Although they were certainly interested in the reforms themselves, the primary goal was to build a powerful coalition of activist women who would help—both directly and indirectly—in achieving suffrage. Once the Federal Amendment passed, these women turned to other, more divisive concerns.[13]

There was probably never quite the consensus among this diverse group of women that Skocpol implies. The differences that had always existed between club members and club leaders were hidden during the suffrage campaign. Women needed to speak in a single, united voice to be effective—and the views of leaders were the most likely to be voiced. There were also distinct regional and class divisions, even among the leadership. As is often the case, a common goal created a shared sense of purpose that allowed these women to ignore their many differences in order to concentrate their efforts. Once the goal was achieved, the differences were again apparent, and this made it difficult to achieve many of the secondary goals of the movement. An ironic twist is that the power of women's votes to influence specific women's issues was actually greater before they possessed it. As legislators gradually became aware that they were unlikely to vote as a bloc, even on "women's issues," the power of female voters to influence legislation declined.

This does not, however, indicate that women gained little from enfranchisement. The value of suffrage for women should not be measured only by the achievement of a "women's platform." The power to vote reinforced links between women and male voters with similar interests, which meant that women began to pursue political objectives through more traditional media, such as parties. It broadened their access to political office and meant that their wishes, as well as men's, were taken into account when the legislative agenda was being set at both the state and the national levels. Finally, suffrage gave women the right to express through their votes the personal views that were often subsumed when their only mode of expression was participation in large, amorphous organizations. Once the coalescing force of the suffrage movement was removed, women ceased to act merely as part of an excluded group and began acting politically as individuals.

It was not women's position outside the system but rather their position on the threshold of the system (with one foot wedged firmly in the door) that enabled them to convince state legislatures to adopt policies such as mothers' pensions. Since many of the women who were lobbying for this policy were, either as part of their women's clubs or their suffrage clubs, also working for the ballot, it seems unlikely that these concerns were entirely separate in the minds of the legislators who heard their arguments for reforms in the same year that they heard their pleas for suffrage. For years, women's organizations presented their concerns to their "representatives" only to receive a polite smile and a quick dismissal. The underlying message was clear: "Why should I listen to you? You can't vote." Indirect influence had achieved little. It was clear to most politically astute women of the era that the ballot was necessary if they were to achieve meaningful reform. Without the leverage provided by the suffrage movement, mothers' pensions would have received the same lukewarm reception as the many other policies sought by organized women in the past.

APPENDIX: STATE-BY-STATE CODING INFORMATION

Table A-1 contains information used in coding our variables for each of the 48 states.

The following summaries contain further details on the coding of our variables SUFFRAGE PRESSURE ON LEGISLATURE and SUFFRAGE CAMPAIGNS for each state in Table A-1 that is followed by an asterisk.

Illinois. Miss Harriet Grim was hired as state organizer in 1910 and worked throughout Illinois during 1910–11 organizing suffrage clubs, seeking endorsements from other organizations, and lobbying the state legislature. Although the referendum bill passed the Senate in 1911, it failed to pass the House by 6 votes. (Harper 1922, 147–50; *Woman's Journal*, 1 April 1922, 97).

Iowa. Governor George Clark (Republican, 1913–17) was an open advocate of woman suffrage and frequently spoke at suffrage rallies (Harper 1922, 183–86; *Woman's Journal* 25 May 1912, 161).

Massachusetts. Governor Eugene Foss (Progressive Democrat, 1911–14) endorsed suffrage. The State Suffrage Association also began actively campaigning against antisuffrage legislators in 1912, resulting in the defeat of one in 1912 and an additional five in 1913 (Harper 1922, 295–97).

Michigan. Governor Chase Osborn (Republican, 1911–13) was a staunch advocate of suffrage, and supported the submission of referenda twice during his administration. The referendum of 1912 was defeated by less than .002% of the vote, amid strong evidence of ballot tampering. The suspicion of fraud resulted in the resubmission of the issue to the voters in 1913. The brewers and distillers poured much more money into the second election due to the fear that suffrage would usher in prohibition. The suffrage measure was defeated in 1913, 61% to 39% (Harper 1922, 306–8).

Minnesota. Governor Adolph Eberhart (Republican, 1909–15) was a supporter of suffrage and also a member of the Men's Advisory Committee to the Minnesota State Suffrage Association. Senator Ole Sageng introduced the bills for both suffrage and mothers' pensions in this legislative session. Although the referendum bill passed the House, it failed to pass the Senate by three votes (*Woman's Journal*, 23 March 1912, 89; *Minnesota Senate Journal* 1913).

Nebraska. The suffrage question was submitted to the voters as the result of a petition campaign conducted in 1913–14 by suffrage organizations. Over 50,000 signatures were obtained, nearly 13,000 more than were required to place the amendment on the 1914 ballot. NAWSA contributed both workers and funds to the campaign, because it was seen as a good test of whether the newly passed initiative and referendum laws in many states could be effective in achieving suffrage (Harper 1922, 373–78; *Woman's Journal*, 29 February 1911, 63).

New Hampshire. Governor Samuel Felker (Democrat, 1913–15) supported suffrage and spoke in favor of the reform at a banquet in February of 1913 (Harper 1922, 404).

New Jersey. The suffrage referendum was held in October of 1915 but was originally planned for 1914. Both houses of the 1913 legislature passed the resolution by a substantial margin. It was then required that the bill was advertised to the public prior to the next meeting of the legislature in 1914. The governor failed to see that the bill was advertised as required, claiming that his secretary had mislaid the bill and forgotten to remind him. The procedure was begun again in 1914 (Harper 1922, 420–32; *Woman's Journal* 25 October 1913, 341–42).

Nevada. The legislature of 1911 had passed the suffrage measure, but the approval of two legislatures was required before submission to the voters. The legislature of 1913 also passed the amendment, and suffrage was granted to women by the voters of the state in November of 1914. NAWSA provided substantial support for the campaign, in the form of cash grants and by paying the salaries of several workers and organizers (Harper 1922, 385–88, 392–99).

Ohio. Ohio held a constitutional convention in 1912, including an amendment granting suffrage to women and an amendment concerning initiative and referendum. The suffrage measure failed, but the initiative and referendum amendment passed. Suffragists then began a petition campaign, resulting in the resubmission of the suffrage measure to the voters in the election of 1914 (Harper1922, 509–13; *Woman's Journal* 13 September 1913, 187; 14 June 1913, 296).

South Dakota. Governor Frank Byrne (Republican Progressive, 1913–17) and his wife were noted as valuable allies in getting the suffrage bill passed by the legislature (Harper 1922, 588–89).

Wisconsin. In 1911, the legislature passed a bill to submit the issue of suffrage to the voters, and the bill was signed by Governor Francis McGovern (1911–15). The referendum of 1912 resulted in defeat of the suffrage amendment, due in part to the strong activity of liquor interests. Both houses of the 1913 legislature determined that the issue should be included on the ballot in 1914, and since the governor had signed the bill in 1911 and his party ("Bullmoose" or Republican Progressive) had declared in favor of suffrage, it was assumed that he would again sign the measure. He did not sign, however, stating that the issue should not be returned to the voters so quickly after they had voted against it. NAWSA was an active force in the 1912 campaign, contributing several thousand dollars and paying the expenses and salaries of numerous out-of-state campaign workers (Harper 1922, 700–703; *Woman's Journal* 7 June 1913, 184; 14 June, 187).

New York. Both houses of the 1913 legislature passed the bill to submit the suffrage question to the voters. In 1915, a second legislature approved the measure, and a referendum was scheduled for 2 November of that year. Given the number of electoral votes held by New York, the NAWSA saw this as a crucial election and was a major financial contributor to the campaign (Harper 1922, 468–75).

North Dakota. Two separate suffrage measures were passed by the legislature of 1913. The first provided for submission of a bill to the voters at the 1914 election that would make woman suffrage a law. The second proposed a constitutional amendment, which would have to pass two legislatures before submission to the voters but would provide greater security since it could not be rescinded as easily. Both measures passed. The 1914 referendum resulted in a narrow defeat of the suffrage law, due to a change in the wording that required it to receive a majority of all voters at the election, not merely of all voters on the particular issue, as was required for other measures to pass. This meant that blank ballots counted against the bill. The proposal for a constitutional amendment faced its second required legislature in 1915, where it passed the Senate by a two-to-one margin and was then referred to the House, where support for the measure was even greater. Opponents in the Senate managed to push through a vote to recall the bill from the House after its second reading and blocked any further consideration during that Legislature. The NAWSA contributed several thousand dollars to the 1914–15 campaign and also paid the expenses of organizers and speakers. Presidential suffrage was granted in 1917 (Harper 1922, 502–6; *Woman's Journal* 14 June 1915, 129).

Tennessee. Governor Thomas Rye (Democrat, 1915–19) announced in favor of suffrage during the 1915 campaign and was helpful in securing the endorsement of the Democratic party. Suffrage organizations were also active in the effort to secure a constitutional convention, including a vote on suffrage, which failed by a narrow margin. Both houses of the 1915 legislature passed a bill to submit the suffrage question to the voters, but the approval of two consecutive legislatures was required. The NAWSA supplied paid organizers in 1913 and 1914 and held its national convention in Nashville in 1914, resulting in additional press coverage and attention for the state suffrage efforts (Harper 1922, 598–600, 608–610; *Woman's Journal* 6 February 1915, 39; 1 May 151).

West Virginia. The legislatures of 1913 and 1915 both passed resolutions submitting the question of suffrage to the voters, and the referendum was scheduled for November 1916. Governor Henry Hatfield (Republican, 1913–17) was a supporter and made speeches in favor of suffrage in 1915. NAWSA began funding the West Virginia campaign in 1915 and supported several organizers, but the bulk of the money spent by NAWSA was expended in the last months of the 1916 campaign (Harper 1922, 689–92).

Maryland. Although there was suffrage activity in Maryland during this period, there does not appear to have been any strong increase in activity, and we could locate no evidence for any of the indicators we used in coding suffrage pressure. The entry for Maryland in Harper 1922 does note, however, that the Maryland Woman Suffrage Association "in connection with its suffrage activities worked in the Legislature for other progressive measures," including the abolishment of child labor, equal pay for equal work, and coeducation (p. 255).

Arkansas. The 1917 legislature granted women the right to vote in all primary elections. The Democratic party was so dominant in Arkansas that elections were largely decided in the primaries, so this form of suffrage was considered the virtual equivalent of full suffrage (Harper 1922, 19).

Delaware. Governor John Townsend, Jr. declared his support for suffrage in his inaugural address in 1917. NAWSA sponsored several state organizers between 1915 and 1917 (Harper 1922, 90–91, 103).

Maine. The legislatures of 1915 and 1917 both passed resolutions to submit a suffrage amendment to the voters. The referendum was scheduled for September 1917. Governor Carl Milliken (Republican, 1917–21) gave his official endorsement in March of that year. NAWSA had agreed to provide support for a campaign if it were delayed until 1918, believing that six months was too brief a period in which to organize a winning campaign. Despite the objections of its president, Carrie Chapman Catt, NAWSA contributed over $15,000 to the 1917 campaign, which lost by a nearly two-to-one margin (Harper 1922, 243–45; *Woman's Journal* 10 March 1917, 55).

Missouri. NAWSA sponsored a congressional conference in Missouri in 1916, which brought together state suffrage organizations, state and national suffrage speakers and organizers, and representatives from each legislative district to discuss the extension of suffrage at both the state and national levels. NAWSA also funded a series of "suffrage schools" in Missouri during 1917, where national organizers and workers from states where suffrage had been gained instructed local suffrage organizations on, among other things, effective methods of lobbying and campaigning (Harper 1922, 347–50; *NAWSA Headquarters Bulletin*, May 1916, p. 5; ibid., April 1917, p. 23).

Texas. The 1917 budget for the Texas Equal Suffrage Association was $30,000—six times larger than that of the previous year. Over half of this sum was set aside to cover the salaries and expenses of eight full-time field workers and organizers. These workers lobbied their representatives on a daily basis during the legislative session of 1917. A bill for primary suffrage received a majority (but not the required two-thirds) in the House but was not considered in the Senate due largely to that body's preoccupation with the pending impeachment of the governor, William Ferguson. (A special session of the legislature, held in February 1918, passed the primary suffrage bill.) NAWSA provided the services of Lavinia Engle, an organizer with experience in southern suffrage work, and sponsored suffrage schools (see entry for Missouri) in the state during 1916–17 (Harper 1922, 632–35; Winegarten and McArthur 1987, 148–53; *NAWSA Headquarters Bulletin*, April 1917, p. 23).

Vermont. The NAWSA sponsored a congressional conference (see entry for Missouri) in March 1916 and sent a worker from NAWSA for one month to help organize the state. The Vermont Woman Suffrage Association hired a state organizer in 1917 (Harper 1922, 654–55; *NAWSA Headquarters Bulletin*, May 1916, p. 5; ibid., April 1917, 23).

Florida. NAWSA supplied numerous organizers to work in Florida during 1918 and 1919, because the state was viewed as one of the few southern states that might vote to ratify the Federal Suffrage Amendment (Harper 1922, 117–8).

Indiana. Presidential suffrage was granted by the legislature in 1917 but was delayed for two years due to questions about its constitutionality. The vote for resubmission of the amendment to the voters took place in 1919 but was tabled due to the pending Federal Amendment.

New Mexico. The state constitution of New Mexico did not allow amendments on suffrage. Thus, there was no suffrage organization in the state. The entry for New Mexico in Harper 1922 notes that the Legislative Department of the Federation of Women's Clubs "really did the work that any suffrage organization would do" (p. 434). The chair of the Legislative Department was also state chair for NAWSA after 1913. The federation managed after 1914 to "secure some legislation favorable to women and children" (prob-

TABLE A-1

Dependent and Independent Variables for 48 States

Year Mothers' Pensions Enacted^a	Suffrage at Time of Pensions Score^b	Year Pensions Endorsed by Women's Group(s)^c	Suffrage Endorsement by State Federation^d	Elements of Suffrage Pressure on Legislature^e	Suffrage Campaigns Score^b	Difference in Innovation on	
						Mothers' Pensions^f	Suffrage^f
1911							
IL* (1913)	0	[1914–15]	1904, 1911(1)	party (P), paid lobbyist/organizer	4	.479	−.288
1913							
CA (1911)	4	1912–13	1903, 1911(1)	voting (constitutional amendment, 1911)	0	.271	−.304
CO (1893)	4	1912–13	1895, 1912(1)	voting (constitutional amendment, 1893)	0	.337	.362
ID (1896)	4	1912–13	[1905, 1912](1)	voting (constitutional amendment, 1896)	0	.481	.406
IA* (1919)	0	1912–13	[1911](1)	referendum bill passes, governor, party (R, P)	4	.462	−.380
MA* (1920)	0	1912–13	1904(0)	governor, party (R, Pb)	4	.246	−.629
MI* (1917)	0	1912–13	1910, 1912(1)	referendum (1913), governor	4	.297	−.511
MN* (1919)	0	1912–13	1908, 1912(1)	governor, party (P)	4	.350	−.492
NE* (1917)	0	no evidence	1912(1)	referendum bill passes, party (R), NAWSA support	4	.450	−.325
NH* (1920)	0	1912–13	1905, 1912(1)	governor, all parties	4	.393	−.482
NJ* (1920)	0	[1914–15]	1904(0)	referendum bill passes, party (P)	4	.290	−.585
NV* (1914)	0	1912–13	1911, 1913(1)	referendum bill passes, all parties, NAWSA support	4	.552	−.123
OH* (1920)	0	1912–13	1912(1)	referendum (by petition), NAWSA support	4	.347	−.495
OR (1912)	4	1912–13	[1906](1)	voting (constitutional amendment, 1912)	0	.331	−.277
PA (1920)	0	1912–13	1902(1)	referendum bill passes, party (D, R, Pb)	4	.315	−.560
SD* (1918)	0	1912–13	1912(1)	referendum bill passes, governor	4	.512	−.296
UT (1895)	4	1912–13	1895, 1912(1)	voting (constitutional amendment, 1896)	0	.428	.386
WA (1910)	4	1912–13	1910, 1912(1)	voting (constitutional amendment, 1910)	0	.365	−.177
WI* (1919)	0	1912–13	1912(1)	referendum bill passes, party (P), NAWSA support	4	.343	−.499
1914							
AZ (1912)	3	no evidence	1902, 1912(1)	voting (constitutional amendment, 1912)	0	.449	−.117
1915							
KS (1912)	3	1914–15	1912(1)	voting (constitutional amendment, 1912)	0	.366	−.159
MT (1914)	3	1914–15	1912(1)	voting (constitutional amendment, 1914)	0	.414	−.178
NY* (1917)	0	1912–13	1909, 1912(1)	referendum (1915), party (D), NAWSA support	3	.136	−.556
ND* (1917)	0	no evidence	1915(1)	referendum (1914), NAWSA support	3	.348	−.344
OK* (1918)	0	1914–15	1910(1)	party (R, P)	3	.424	−.301
TN* (1919)	0	1912–13	1916(0)	referendum bill passes, governor, NAWSA support	3	.403	−.356
WV* (1920)	0	1914–15	1915(1)	referendum bill passes, governor, NAWSA support	3	.406	−.386
WY (1890)	3	1914–15	1904, 1912(1)	voting (1890)	0	.446	.654
1916							
MD* (1920)	0	1916–17	[1920](0)	no evidence	0	.268	−.482
AR* (1917)	2	no evidence	1915(1)	voting (primary suffrage)	0	.314	−.294
DE* (1920)	0	1914–15	1920(0)	governor, NASWA support	2	.329	−.376
ME* (1919)	0	1916–17	[1917](1)	referendum (1917), governor, party (D)	2	.253	−.422
MO* (1919)	0	1914–15	1914(1)	party (D), NAWSA support	2	.331	−.377
TX* (1918)	0	1914–15	[1915](1)	paid lobbyists/organizers (8), NAWSA support	2	.346	−.295

TABLE A-1 continued

Dependent and Independent Variables for 48 States

Year Mothers' Pensions Enacted[a]	Suffrage at Time of Pensions Score[b]	Year Pensions Endorsed by Women's Group(s)[c]	Suffrage Endorsement by State Federation[d]	Elements of Suffrage Pressure on Legislature[e]	Suffrage Campaigns Score[b]	Difference in Innovation on	
						Mothers' Pensions[f]	Suffrage[f]
1916 (continued)							
VT* (1920)	0	1916–17	1914(1)	party (D, R), paid organizer, NAWSA support	2	.294	−.414
1918							
VA (1920)	0	no evidence	1919(0)	party (R), 19th Amendment pending	1	.216	−.451
1919							
CT (1920)	0	1918–19	no evidence(0)	party (D, R), 19th Amendment pending	1	.057	−.568
FL (1920)	0	1916–17	1915(1)	NAWSA support, 19th Amendment pending	1	.228	−.397
IN* (1919)	1	1918–19	1914(1)	voting (presidential 1917, reenacted 1919)	0	.161	−.431
1920							
LA (1920)	0	no evidence	1915(1)	voting (19th Amendment, Did not ratify)	0	.125	−.459
1923							
NC (1920)	0	no evidence	1918(0)	voting (19th Amendment, Did not ratify)	0	.028	−.430
RI (1917)	0	no evidence	no evidence(0)	voting (presidential 1917, 19th Amendment)	0	−.045	−.403
1928							
KY (1920)	0	no evidence	1912(0)	Voting (19th Amendment ratified)	0	−.169	−.419
MS (1920)	0	1918–19	[1916](0)	voting (19th Amendment, Did not ratify)	0	−.048	.298
1931							
AL (1920)	0	no evidence	no evidence(0)	voting (19th Amendment, Did not ratify)	0	−.239	−.406
NM* (1920)	0	no evidence	1914(0)	voting (19th Amendment ratified)	0	−.208	−.375
Without Laws in 1935							
GA (1920)	0	no evidence	1919(0)	voting (19th Amendment, Did not ratify)	0	−.381	−.381
SC (1920)	0	no evidence	1916(0)	voting (19th Amendment, Did not ratify)	0	−.347	−.347

Sources: Harper 1922; *Woman's Journal* 1911–17; *Woman Citizen* 1917–19; *NAWSA Bulletin; NAWSA Records*; Skocpol et al. 1993; Skocpol 1992.

[a] The year suffrage was granted is in parentheses. States that are underlined were especially innovative on mothers' pensions.

[b] The scores for suffrage at time of mothers' pensions and suffrage campaigns (Table 1) are both coded on an ordinal scale similar to that used by Skocpol et al. for their dependent variable and women's group endorsements. suffrage at time of mothers' pensions looks only at those states where women were allowed to vote when mothers' pensions were adopted. Those states that met this criteria and adopted mothers' pensions between 1911 and 1913 were coded as 4; 1914–15, 3; 1916–17, 2; 1918–19, 1; and those adopting after the enactment of the Nineteenth Amendment are coded as 0. Those states where suffrage had not been granted prior to the enactment of mothers' pensions are also coded as 0. suffrage campaigns is coded on the exact same scale and measures whether there was a significant suffrage campaign in progress in the state during the two years prior to the enactment of mothers' pensions. The dichotomized version of suffrage campaigns that we use in our final model gives a score of 1 to those states where the legislature was subjected to one or more of the elements of suffrage pressure, as listed in that table, during the two years prior to the enactment of mothers' pensions. All other states are scored as 0.

[c] As listed in Skocpol et al. 1993. Brackets indicate those cases where endorsement came after the initial legislation was passed. This information is used in the coding of women's group endorsements and in our dichotomized version of that variable—club endorsements—included in our final model. As in Skocpol et al. 1993, those states where women's clubs endorsed between 1911 and 1913 are coded as 6; 1914–15, 5, 1916–17, 4; 1918–20, 3; 1923–28, 2; and 1931–34, 1. The two states that did not enact this legislation prior to the Social Security Act of 1935 are coded as 0. Our dichotomized version of this variable gives those states where women's groups endorsed mothers' pensions during the two years prior to their enactment a score of 1. All other states receive a score of 0. See also Skocpol 1992, table 9 and app. 2.

[d] The first date indicates initial endorsement by the State Federation of Women's Clubs. The second date is the most recent endorsement prior to enactment of mothers' pensions. Brackets indicate the states in which affiliates of the State Federation of Women's Clubs do not appear to have played a major role in the passage of mothers' pensions. The number in parentheses indicates the score used in coding the dichotomous variable, suffrage activity in women's clubs, included in our final model. Those states that had endorsed–or were actively supporting—the extension of suffrage (either at the state level or through the federal amendment) when mothers' pensions were enacted in that state are coded as 1, and all other states receive a score of 0. (Two state federations, Massachusetts and New Jersey, endorsed suffrage early but were divided on the issue in 1913, the year that these states passed mothers' pensions. Therefore, they are coded as 0.) Those states that passed mothers' pensions after 1920 are also coded as zero, as they could not be endorsing the extension of suffrage once full suffrage was granted via the Nineteenth Amendment.

[e] R = Republican, D = Democratic, P = Progressive, Pb = Prohibition.

[f] difference in innovation in mothers' pensions and difference in innovation on suffrage are derived from the composite innovation score in Walker 1969 and 1971.

ably dealing with compulsory education and child labor, see Porrit 1916), but mothers' pensions were apparently never requested.

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RESPONSE

The early 1900s represented a high point of legislative influence for many women in the United States. Spurred by intergroup coalitions, state legislatures and the U.S. Congress passed new laws designed to benefit women as mothers, workers, and citizens. As the following list shows, key pieces of social legislation featuring women were prominent among such innovations (from Skocpol 1992, 387–90, 402–3, 446–47, 480–81):

1900–1909	New or improved women's hour laws passed in 17 states
1910–17	New or improved women's hour laws passed in 35 states Mothers' pensions passed in 35 states Minimum wage laws for women passed in 12 states
1912	U.S. Children's Bureau established
1918–20	Mothers' pensions passed in 5 states, minimum wage laws for women passed in 2 states
1918	U.S. Women's Bureau established
1921	Sheppard–Towner "Federal Act for the Promotion of the Welfare and Hygiene of Maternity and Infancy"
1921–31	Mothers' pensions passed in 6 states, minimum wage law for women passed in 1 state

Woman suffrage was also instituted. The Nineteenth Amendment to the U.S. Constitution was passed by Congress in June 1919 and was quickly ratified by three-quarters of the states by 26 August 1920. Prior to that, as the following list details, 30 out out 48 states had enfranchised their female citizens to some degree (adapted from Scott and Scott 1975, 166–68).

1890	**Wyoming** was admitted to statehood with woman suffrage, having had it as a territory since 1869.
1893	**Colorado** adopted a constitutional amendment after defeat in 1877.
1896	**Idaho** adopted a constitutional amendment on its first submission.
1896	**Utah**, after having woman suffrage as a territory since 1870, was deprived of it by the Congress in 1887, but by referendum put it back in the constitution when admitted to statehood.
1910	**Washington** adopted a constitutional amendment after defeats in 1889 and 1898. It had twice had woman suffrage by enactment of the territorial legislature and lost it by court decisions.
1911	**California** adopted a constitutional amendment after defeat in 1896.
1912	**Oregon** adopted a constitutional amendment after defeats in 1884, 1900, 1906, 1908, and 1910.
1912	**Kansas** adopted a constitutional amendment after defeats in 1867 and 1893.
1912	**Arizona** adopted a constitutional amendment submitted as a result of referendum petitions.
1913	**Illinois** was the first state to get presidential suffrage by legislative enactment.
1914	**Montana** adopted a constitutional amendment on its first submission.
1914	**Nevada** adopted a constitutional amendment on its first submission.
1917	**North Dakota** secured presidential suffrage by legislative enactment, after defeat of a constitutional amendment in 1914.
1917	**Nebraska** secured presidential suffrage by legislative enactment after defeats of a constitutional amendment in 1882 and 1914.
1917	**Rhode Island** secured presidential suffrage by legislative enactment after defeat of a constitutional amendment in 1887.
1917	**New York** adopted a constitutional amendment after defeat in 1915.
1917	**Arkansas** secured primary suffrage by legislative enactment.
1918	**Michigan** adopted a constitutional amendment after defeats in 1874, 1912, and 1913. Secured presidential suffrage by legislative enactment in 1917.
1918	**Texas** secured primary suffrage by legislative enactment.
1918	**South Dakota** adopted a constitutional amendment after six prior campaigns for suffrage had been defeated.
1918	**Oklahoma** adopted a constitutional amendment after defeat in 1910.
1919	**Indiana** secured presidential suffrage by legislative enactment in 1917. Rendered doubtful by a court decision, the law was reenacted with by six dissenting votes.
1919	**Maine** secured presidential suffrage by legislative enactment after defeat of a constitutional amendment in 1917.
1919	**Missouri** secured presidential suffrage by legislative enactment after defeat of a constitutional amendment in 1914.
1919	**Iowa** secured presidential suffrage by legislative enactment after defeat of a constitutional amendment in 1916.
1919	**Minnesota** secured presidential suffrage by legislative enactment.
1919	**Ohio** secured presidential suffrage by legislative enactment after defeat of referendum on the law in 1917 and of a constitutional amendment in 1912 and 1914.

1919 **Wisconsin** secured presidential suffrage by legislative enactment after defeat of a constitutional amendment in 1912.
1919 **Tennessee** secured presidential suffrage by legislative enactment.
1920 **Kentucky** secured presidential suffrage by legislative enactment.

Why did legislative enactments demanded by—and benefitting—American women occur in such profusion during the early 1900s? The debate between Sparks and Walniuk and my associates and I speaks to this overall question (see Skocpol 1992; Skocpol et al. 1993). Yet the proximate empirical focus of our debate is on mothers' pensions—enacted by 20 states in 1911–14, by 15 more in 1915–17, by five more in 1918–20, and by six more between 1923 and 1931. The pattern and priority of these enactments are challenging to explain, not least because most states adopted mothers' pensions before their female citizens could vote. After all, as the suffrage list documents, prior to 1917 only 12 out of 48 states had enfranchised women (and one of those states, Illinois, allowed female voting only for presidential electors). Perhaps most telling, before 1921 most state legislators (as well as representatives in Congress) did not have to to face female voters, because only 17 (mostly small) states enfranchised women in other than presidential elections prior to the Nineteenth Amendment.[14]

Then or now, however, voting is far from the only kind of political power that matters. Whether they could yet vote back in the 1910s or not, many American women enjoyed another crucial kind of political influence. They used voluntary civic mobilization to agitate on behalf of social legislation—and often on behalf of the suffrage as well. By the late nineteenth and early twentieth century, American women all across the country were members of local voluntary groups. These groups, moreover, had formed *widespread, federated associations,* including the Women's Christian Temperance Union, the National Congress of Mothers, the GFWC, and also the major prosuffrage organizations. Because women's federated associations cut across all of the states and because they had an organized presence in individual communities and legislative districts, they were able to influence nationwide agendas of public discussion. Legislators and publics across many states were *simultaneously* persuaded that certain social problems and legislative proposals were worthy of attention. As the contempory political science literature about "agenda setting" has amply demonstrated, creating a sense of an idea "whose time has come" counts for a lot in public policymaking (Baumgartner and Jones 1993; Kingdon 1984; Rochefort and Cobb 1994). At a time in U.S. history when the major political parties were not programmatically coherent, when legislatures were not professionalized, and when federal bureaucracies were barely established, U.S. women's federations exercised a surprising influence on legislatures and public opinion. Contrary to what Sparks and Walniuk claim, our argument was not about women acting "outside the political system" (this phrase is entirely their own invention). Rather, we focused on a different kind of political capacity, namely, civic mobilization for agenda setting.

Counterposed to the *civic mobilization/agenda setting* argument that my associates and I put forward is the stress on actual or anticipated *suffrage pressure* by Sparks and Walniuk. Noting that the American woman suffrage movement was at its peak in the 1910s, they hypothesize that "legislators [in the various states] were more amenable to the legislation that women favored, including mothers' pensions, when they were faced with either the reality of female voters or the strong possibility of female voters in future elections" (p. 710). Following current fashion in rational choice analysis, they theorize speculatively about what might have been going in the minds of the legislators who enacted mothers' pensions; and they indicate the pattern of state enactments that might have resulted if guesses about the fears and incentives facing legislators are true:

> The focused action of women who lobbied the legislature for both reforms and suffrage led to a general perception among legislators that women might vote as a bloc. . . . The fear of a bloc vote is cited by Flexner (1975, 338) as a key reason why the issues that women's groups were promoting were most effective shortly after winning suffrage and then declined throughout the latter half of the 1920s as it became increasingly clear that women's votes were not following a gender-dictated pattern. . . . Anticipation of a bloc vote may also be a reason why the issues that women endorsed were especially effective in the period just prior to suffrage, because legislators had to consider the possibility that their electorate could be doubled in the upcoming elections by the addition of women. (p. 712)

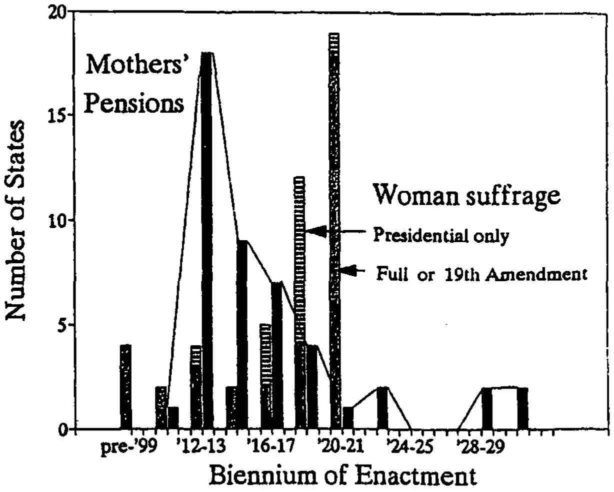
In states where women were not yet voting in the 1910s, they maintain that legislators might especially have anticipated women's vote in the near future if they faced "strong" campaigns in favor of woman suffrage.

AN INITIAL PLAUSIBILITY TEST

To explore the applicability to mothers' pensions of the sort of reasoning offered by Sparks and Walniuk, we can first use simple modeling. If, as they maintain, the state legislators who voted for mothers' pension laws were principally responding to the actuality or palpable probability of female votes, then we should see a pattern of mothers' pension enactments resembling the bars for (full plus partial) woman suffrage in Figure 1. Mothers' pension enactments should have accumulated slowly after the proposal first came onto the national agenda in 1911, reflecting the fact that only a quarter of the states had woman suffrage by 1914. After 1915, according to Sparks and Walniuk's reasoning, mothers' pension enactments should have built to a crescendo of more than two dozen enactments in the years before and

FIGURE 1

Mothers' Pensions and Woman Suffrage



Sources: Scott and Scott 1975, 166–68; Skocpol 1992, 446–47, table 9.

did not then rush to do so. Further enactments dribbled out over the next decade; and two states (whose women had been voting for 15 years) still had not established mothers' pensions when the federal government transformed the program into Aid to Dependent Children in 1935.

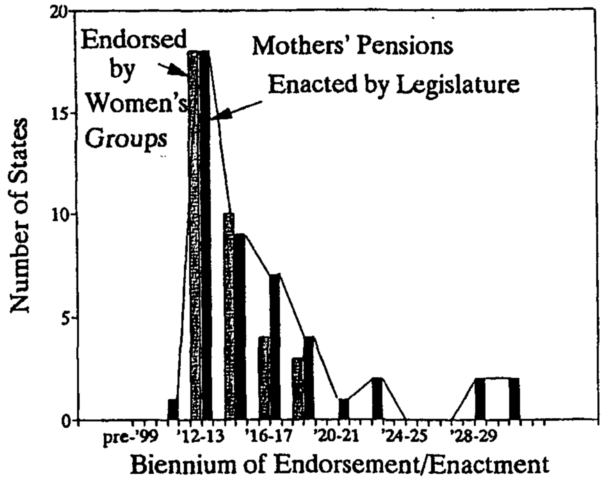
As Figure 2 shows, the Skocpol reasoning about the agenda-setting capacities of widespread federations of women's voluntary groups much more closely parallels the actual timing and aggregate patterns of mothers' pension enactments. Mothers' pensions were first featured as a nationwide legislative goal in 1911 by the National Congress of Mothers (a federation that did not endorse suffrage), and they were also endorsed in 1912 by the GFWC (which endorsed the suffrage in 1914). After 1911–12, the local and state affiliates of these federations led campaigns for mothers' pensions throughout the 1910s, usually endorsing them right before enactments by the relevant state legislatures. (For a full discussion of the relevant developments, see Skocpol 1992, chap. 8.)

just after 1920. This pattern would be expected because 18 states achieved some sort of woman suffrage between 1917 and 1920; and the 18 remaining nonsuffrage states should have enacted mothers' pensions just before or right after 1920, when they were faced with actual women voters.

But the bunching of mothers' pension enactments before and just after 1920 that Sparks and Walniuk's reasoning would lead us to expect is very different from what actually happened historically (as depicted by the solid black bars in Figure 1). In reality, 20 states very suddenly enacted mothers' pensions in 1911–14, and 15 more enacted them by 1917. What is more, the states that had not enacted mothers' pensions by 1920

FIGURE 2

Mothers' Pensions and Women's Group Endorsements



Source: Skocpol 1992, 446–47, table 9.

CONTRASTING REGRESSION FINDINGS

Although Figure 1 shows that Sparks and Walniuk's reasoning does not correspond to aggregate patterns of mothers' pension enactments across the states, perhaps actual or anticipated suffrage interacted with other causal factors in ways that only multiple regression can discern. In the statistical research my associates and I did (for Skocpol et. al. 1993), we carefully considered this possibility. Probing for the possible effects of many independent variables on the priority of mothers' pension enactments, we included an assessment of various alternative independent variables referring to woman suffrage. Like Sparks and Walniuk, we hypothesized that many states during the 1910s might have had electoral or legislative climates relatively favorable to women's suffrage even if women were not yet officially enfranchised. After exploring various alternatives, we developed a variable that tried to capture not just actual voting by women but also whether each state seemed generally predisposed toward woman suffrage prior to 1920. Using data reproduced here in the list on pages 720 and 721, we coded a state 2 if it had, on its own, enacted full woman suffrage at any time prior to 1920. States were coded 1 if they had enacted one or another version of partial female suffrage prior to 1920; and states got a 0 only if they enacted no form of suffrage prior to 1920.

Our variable gave a very favorable hearing to the kinds of anticipatory processes that Sparks and Walniuk believe might have operated in legislators' minds. Taken alone, this early suffrage variable had some capacity to account for variation in how early states enacted mothers' pensions.[15] But it did not remain "in the running," statistically speaking, when entered into models in competition with other potential independent variables referring to a variety of electoral, socioeconomic, and institutional conditions that varied across the states. Our early-suffrage variable was tested in interaction with other independent variables right up to the end of our study; it was given every chance to prove significant as a partial explanation for the overall ordering of mothers' pension enactments.

Because of the careful consideration that we had given to ideas about the effects of women's suffrage and state-level climates favorable to it, I wondered, when made aware of them, how Sparks and Walniuk's extraordinarily strong statistical claims possibly be true.

COOKING THE BOOKS: TABLE 1 IS A TAUTOLOGY

It turns out to be easy to show that Sparks and Walniuk have *not* made their case if one is willing to read tables and notes much more carefully than people usually do, especially when they want to believe the argument at hand.[16] The fundamental problem is that the two key variables in the crucial Table 1 (SUFFRAGE AT THE TIME and SUFFRAGE CAMPAIGNS) were improperly constructed in such a way as to jointly incorporate the variation in the dependent variable that Sparks and Walniuk claim to account for in this table (i.e., the timing of state-level mothers' pension enactments). As any student of elementary statistics knows, it is wrong to code independent variables in terms of values of the dependent variable. This can produce apparently very "significant" statistical results, but they amount to nothing more than a tautology. Drawing carefully on what Sparks and Walniuk reported about their variable operationalizations (in their text, notes, and appendices), I have assembled in Table 3 all of the information that the reader needs to understand exactly how their tautological variable constructions were accomplished.

Let us start with what Table 3, columns a and b, show us about the construction of Sparks and Walniuk's independent variable, SUFFRAGE AT THE TIME. Theoretically, the aim here is to tap the electoral incentives that legislators faced—or did not face—from actual women voters. In Sparks and Walniuk's own words, this variable supposedly answers the simple "question of whether women were already voting in the state when mothers pensions passed." Ignoring the reality of partial woman suffrage, Sparks and Walniuk posit that states with any kind of suffrage at the time of enactment of mothers' pensions should be scored 1 for *yes*, while those with no suffrage at that time should be scored 0 for *no*. Table 3, column a, records the 1s and 0s that should be assigned according to this direct logic. The reader can easily see that the directly measured SUFFRAGE AT THE TIME variable bears little relationship to the timing of state enactments of mothers' pensions.

But look at column b to see how Sparks and Walniuk manipulated the direct (1 or 0) measures for SUFFRAGE AT THE TIME before they entered this variable into their regression analysis. For one thing, they arbitrarily assigned all states that delayed suffrage until the Ninteenth Amendment a zero value on this variable. According to their blatant miscoding, the six states that enacted mothers' pensions between 1923 and 1931 are recorded as having "no suffrage" when pensions passed even though women actually were voting at the relevant times in all of those states (as they were also voting after 1920 in the two states that did not enact pensions at all). At the same time, they gave inflated *yes* scores (4 or 3 or 2) to those states that had suffrage when they enacted mothers' pensions prior to 1918. The earlier a suffrage state passed mothers' pensions, the higher its score.

The effect of all of these manipulations is to assign "suffrage at the time of mothers' pensions" a very inconsistent value across states, in a way that *by definition* ensures a strong correlation with the dependent variable. As the reader can easily see, the directly measured variable in column a (which has no relationship to how early a state enacted mothers' pensions) is artificially transformed in column b into the variable that Sparks and Walniuk used—one that now changes in magnitude along with the dependent variable.

Sparks and Walniuk are not only interested in the effects of actual woman suffrage. Indeed, the potential value of their work lies in their attempt to operationalize a much more interesting independent variable, SUFFRAGE CAMPAIGNS. They were not able to come up with a comprehensive measure of political forces favoring and working against women's suffrage in each state during the 1910s. The road to suffrage was constitutionally and politically different from state to state, and balances of power constantly shifted between those favoring or opposing legislation or referenda. Arguing that they could not measure all of that, Sparks and Walniuk instead looked at each state only in the two years prior to its enactment of mothers' pensions. For that period (which, of course, was inconsistent across states), they looked only at prosuffrage developments, adding up a possible five to eight indicators of pressures favorable to woman suffrage at that time.[17] Here are the factors they looked for:

> Was a suffrage referendum pending? A state could get one point if the answer is *yes*.
>
> Did one or more "major" parties in the state endorse suffrage? They count endorsements by the Progressive and Prohibition parties as well as the Democrats and Republicans, and allow states to achieve multiple points on this indicator.[18]
>
> Did the governor endorse suffrage? A state gets one point if the answer is *yes*.

TABLE 3

How Sparks and Walniuk Constructed Their Key Variables for Table 1.

MOTHERS' PENSIONS ENACTED	SUFFRAGE AT TIME		SUFFRAGE CAMPAIGNS			COMBINED FOR TABLE 1
	(A) DIRECT[a]	(B) TRANSFORMED	(C) DIRECT[b]	(D) STRONG?[a]	(E) TRANSFORMED	(F) (B + E)
1911						
Illinois	0	0	2	1	4	4
1913						
California	1	4	—	—	0	4
Colorado	1	4	—	—	0	4
Idaho	1	4	—	—	0	4
Iowa	0	0	4	1	4	4
Massachusetts	0	0	3	1	4	4
Michigan	0	0	2	1	4	4
Minnesota	0	0	2	1	4	4
Nebraska	0	0	3	1	4	4
New Hampshire	0	0	2	1	4	4
New Jersey	0	0	2	1	4	4
Nevada	0	0	3	1	4	4
Ohio	0	0	2	1	4	4
Oregon	1	4	—	—	0	4
Pennsylvania	0	0	4	1	4	4
South Dakota	0	0	2	1	4	4
Utah	1	4	—	—	0	4
Washington	1	4	—	—	0	4
Wisconsin	0	0	3	1	4	4
1914						
Arizona	1	3	—	—	0	3
1915						
Kansas	1	3	—	—	0	3
Montana	1	3	—	—	0	3
New York	0	0	3	1	3	3
North Dakota	0	0	2	1	3	3
Oklahoma	0	0	2	1	3	3
Tennessee	0	0	3	1	3	3
West Virginia	0	0	3	1	3	3
Wyoming	1	3	—	—	0	3
1916						
Maryland	0	0	0	0	0	0
1917						
Arkansas	1	2	—	—	0	2
Delaware	0	0	2	1	2	2
Maine	0	0	3	1	2	2
Missouri	0	0	2	1	2	2
Texas	0	0	2	1	2	2
Vermont	0	0	4	1	2	2
1918						
Virginia	0	0	1 + 19th	1	1	1
1919						
Connecticut	0	0	2 + 19th	1	1	1
Florida	0	0	1 + 19th	1	1	1
Indiana	1	1	—	—	0	1

continued

Were one or more full-time lobbyists or organizers employed by prosuffrage forces in the state? A state gets one point if the answer is *yes*.

Did suffragists in the state receive financial assistance from the National American Woman Suffrage Assocation? A state gets one point if the answer is *yes*.

Finally, even if a state had no indigenous prosuffrage factors, Sparks and Walniuk assign it the equivalent of an extra point if it passed mothers' pensions in 1918 or 1919. They argue that because the Nineteenth Amendment was then pending, legislators necessarily had to anticipate that women would soon be voting.

TABLE 3 continued

How Sparks and Walniuk Constructed Their Key Variables for Table 1.

MOTHERS' PENSIONS ENACTED	SUFFRAGE AT TIME (A) DIRECT[a]	SUFFRAGE AT TIME (B) TRANSFORMED	SUFFRAGE CAMPAIGNS (C) DIRECT[b]	SUFFRAGE CAMPAIGNS (D) STRONG?[a]	SUFFRAGE CAMPAIGNS (E) TRANSFORMED	COMBINED FOR TABLE 1 (F) (B + E)
1920						
Louisiana	1	0	—	—	0	0
1923						
North Carolina	1	0	—	—	0	0
Rhode Island	1	0	—	—	0	0
1928						
Kentucky	1	0	—	—	0	0
Mississippi	1	0	—	—	0	0
1931						
Alabama	1	0	—	—	0	0
New Mexico	1	0	—	—	0	0
No law by 1935						
Georgia	1	0	—	—	0	0
South Carolina	1	0	—	—	0	0

[a] 1 = yes, 0 = no.
[b] 0–8 factors favoring suffrage in two years prior to mothers' pensions.

Table 3, column c, records the direct number of favorable points that Sparks and Walniuk gave each state according to these directly measured indicators for their SUFFRAGE CAMPAIGNS variable. (For this column, I have entered dashes for all early suffrage states and for all states that waited to enfranchise women until after the Nineteenth Amendment. Sparks and Walniuk do not define this variable for those states.) The reader can easily see that Table 3, column c, bears little relationship to the order in which states enacted mothers' pensions. Some states had lots of prosuffrage factors when they legislated; others had only a couple (from Sparks and Walniuk's extraordinarily flexible list).

But Sparks and Walniuk did not proceed with this directly measured set of prosuffrage indicators (column c). Instead, they performed two different transformations on the direct scores, prior to arriving at the variable values (column e) actually used in the regression analysis reported in Table 1. The first transformation (column d) created a dichotomous score in answer to the question whether each state had "strong" prosuffrage forces. Any state with two or more of the possible eight favorable indicators is scored 1 for *yes* on strength, whereas a state that had fewer than two favorable indicators is scored 0 for *no*. The reader should notice, however, that Sparks and Walniuk set an extremely low threshold for prosuffrage "strength"—so low, in fact, that *virtually all variation obtained by their direct measurement of prosuffrage factors was suppressed.* Every single relevant state except Maryland ends up getting a score of 1/*yes* for "strength" in column d. In short, Sparks and Walniuk claim that all but one of the states for which the CAMPAIGNS variable is measured had "strong" prosuffrage campaigns in the two years prior to enacting mothers' pensions. Substantively, Sparks and Walniuk are saying in column d that there was considerable prosuffrage pressure during the 1910s in almost all of these states.

This may be true, but it will not help us explain why some of these states enacted mothers' pensions much earlier than others. Consequently, Sparks and Walniuk do yet another transformation to arrive at the final quantitative values for their SUFFRAGE CAMPAIGN variable. They do not rest content with *yes* and *no* on the question whether there was a strong suffrage campaign in each state. Rather, they multiply the 1/*yes* answers recorded in column d by 4, 3, 2, or 1, according to how early the state in question passed mothers' pensions. Just as happened with the SUFFRAGE AT THE TIME VARIABLE, this manipulation has the effect of building variation on the dependent variable *by definition* into the SUFFRAGE CAMPAIGNS variable. Suddenly, the final SUFFRAGE CAMPAIGNS variable (in column e) appears to "explain" the priority of mothers' pensions!

Obvious historical absurdities become visible as a result of the manipulations that Sparks and Walniuk go through to get from the directly counted suffrage-pressure indicators in column c to the actual variable codings in column e. One need not be a statistical genius (or even particularly statistically literate) to see that very peculiar things are going on. Certain states with only slight evidence of prosuffrage strength end up being assigned very inflated scores on the SUFFRAGE CAMPAIGNS variable in column e, provided that such states passed mothers' pensions early. For example, even though a 1911 attempt to call a suffrage referendum failed to pass the Illinois House, that state is assigned by Sparks and Walniuk with the highest possible SUFFRAGE CAMPAIGNS value of 4—in return for having one paid suffrage organizer working in the state in 1910–11 plus an endorsement of suffrage by the Progressive party (a third party that never had any chance to come to power in Illinois). Clearly, Illinois receives such a high variable score merely because it passed mothers' pensions in 1911. Similarly, Minnesota gets the highest possible score, because it, too, passed mothers pensions very early. Yet Minnesota was a state that delayed suffrage until 1919; in 1912–13 it merely had the governor's support and an endorsement of suffrage by the Progressive party. States with exactly the same number of directly counted suffrage campaign indicators (in column c) as Illinois and Minnesota get lower final scores (in column e) if they happened to have passed mothers' pensions later. Worse, states with significantly stronger prosuffrage profiles in column c (e.g., Maine and Vermont) receive lower final scores than Illinois and Massachusetts in column e for no other reason than that Maine and Vermont enacted mothers' pensions later.

Sparks and Walniuk also arbitrarily differentiate between states that lagged after 1917 in passing mothers' pensions. They assign SUFFRAGE CAMPAIGNS scores of 1 to states that enacted pensions in 1918–19, despite the fact that all three such states had intrastate suffrage factors that added up to fewer than two (their cutoff for a "strong" prosuffrage campaign). The only rationale they offer is that the federal Nineteenth Amendment was pending at that point, a situation that supposedly pressured these states from the outside. But they ignore federally generated suffrage pressures on other states that lagged (until after 1919) in enacting mothers' pensions, automatically assigning 0s to all those states. These illogically differential treatments of states that enacted mothers' pensions from 1918 through 1931 reinforce the differential and often blatantly inconsistent scores that they gave pre-1917 enactors. And both sets of measurement oddities work together to create *the entirety* of the apparent "causal" effect in Table 1 of SUFFRAGE CAMPAIGNS on the relative priority of state mothers' pension enactments.

Why in the world did Sparks and Walniuk go through so many convolutions to construct their SUFFRAGE CAMPAIGNS variable? Why didn't they construct an honest numerical scale for their variable that referred only to suffrage campaign factors, not to the timing of mothers' pension enactments? I confess to being mystified about this.[19] The sad thing is that there was an easy way for them to proceed, given the richness of their historical data. The sensible thing to do was to measure the SUFFRAGE CAMPAIGNS variable for the 1910–12 period across all 48 states. This was just as the twentieth-century suffrage drives were gathering real steam across America. Appropriately, it was also the time just before mothers' pensions spread across the states. A *consistent* measurement of prosuffrage forces across all 48 states for 1910–12 could tell us something meaningful about the role of those forces in promoting dozens of mothers' pension enactments from 1913 onward. Were states that had stronger prosuffrage forces in 1910–12 likely to enact mothers' pensions sooner rather than later? Moreover, it would be easy to incorporate early suffrage into an analysis along these lines. Only seven states had woman suffrage prior to 1913, and they could have legitimately been assigned the highest possible value on a suffrage campaigns scale. After all, states that already had women's suffrage by 1912 either had very strong campaigns during the 1910–12 period (e.g., California, Washington, and Oregon) or else had once had such campaigns at an earlier historical point (e.g., the four Western states that enacted suffrage back in the nineteenth century). In either case, those states had demonstrated strong prosuffrage campaigns prior to the time the mothers' pensions came up for debate in 1911.

If Sparks and Walniuk had proceeded along methodologically legitimate lines such as the forgoing, their ideas about pressure from women's suffrage campaigns could have been given a fair test. But as matters stand, these authors operationalized their preferred new independent variables (SUFFRAGE AT TIME and SUFFRAGE CAMPAIGNS) in obviously invalid and inconsistent ways and, moreover, in ways that tautologically produced the statistical findings that appear in Table 1. By looking at columns b, e, and f in Table 3, the reader can see how Sparks and Walniuk got the results they report for their Table 1: they artifactually created offsetting independent variables defined in terms of the dependent variable. With the single exception of Maryland, any state that got a 0 on one of their two key variables was assigned a positive value on the other, with all the positive scores by definition perfectly chronologically calibrated to reflect how early each state enacted mothers' pensions. Naturally, their two preferred independent variables in Table 1 ended up showing nearly equal beta weights; and together they appeared to account for an overwhelming amount of the variation in timing of mothers' pension enactments. How could it have turned out otherwise, given the way Sparks and Walniuk manipulated the scores on their preferred variables to parallel the dependent variable?

AVOIDING THE ISSUE IN TABLE 2

What about the statistical results that Sparks and Walniuk sum up in Table 2? Whatever these results mean, they are not about the spread of mothers' pensions across the states in the 1910s. The reader will recall that for this part of their analysis, Sparks and Walniuk introduce a radical change in the dependent variable. No longer do they try to explain how early various states enacted mothers' pensions. Instead, they create a dependent variable whose (rather obscure) meaning can be understood only against the backdrop of Jack Walker's (1969, 1971) research on legislative "innovativeness" across the states. To calculate his index of overall state legislative "innovativeness," Walker looked at 88 laws passed over a

150-year period. He did not, however, include mothers' pensions or any other prowomen's laws of the 1910s; nor did he include electoral/political reform measures that swept across the states in those same years. Walker included in his sample a hefty proportion of laws focused on the economy, and for these types of laws he found that New York and other wealthier, more industrialized, and more urbanized states tended to be in the vanguard.

But the most industrialized and urbanized states were *not* in the lead for mothers' pensions—or for other women's and reform measures of the early twentieth century, including early woman suffrage prior to 1915. As anyone can see by looking at Tables 1 and 2, Western and Rocky Mountain states were often the pioneers for women's legislation in the late nineteenth and early twentieth centuries.

Why rearrange an outcome to be explained (e.g., the priority of state enactments of mothers' pensions) in terms of a highly aggregated index that was neither derived from, nor applies well to, the type of legislation and the time period in question? Why, indeed? According to Sparks and Walniuk, it is important to redefine the straightforward dependent variable that we used to measure the relative priority of mothers' pensions enactments between 1911 and 1935, because our model "fails to control for state variations." I cannot imagine what this means in reference to a study that systematically explored 24 different variables tapping a huge range of theoretically relevant variables *measured for the 1910s* across the 48 states. Among the variables we tested were variations that actually existed back then in the relative levels of state urbanization and industrialization. Our variables about these matters correlated powerfully with Walker's innovation index (we actually checked this), so our findings about them also apply to his index.

Sparks and Walniuk acknowledge in note 10 that "one might argue that the model presented by Skocpol and her associates already considers the control factors that account for state variation (e.g., industrialization and modernization [sic]), and rejects these as insignificant" To this obvious point, they reply that we never consider urbanization and industrialization as "control variables in the final model." But they are dead wrong about this. As we explained clearly, we worked with all of our 24 key variables as either potential operative or control variables all the way through the analysis until the end (Skocpol et. al. 1993, 694, and n. 8). Our final models emerged from tests that included INDUSTRIALIZATION, the Walker-style modernization variable that had the strongest relationship to the relative timing of mothers' pensions. In effect, we showed that Walker's index has little explanatory significance. So why should it suddenly be turned into a defining measure for the timing of mothers' pensions enactments? To do this is merely to create a mysterious dependent variable, referring to nothing meaningful or reliable in the real world.

What Sparks and Walniuk's table may actually show is that *very early* state enactors of woman suffrage (i.e., prior to 1915) overlapped to some significant degree with *very early* state enactors of mothers' pensions. This is true. But it does not mean that the timing of suffrage and mothers' pension enactments are closely related overall across the states, because recoding these variables in terms of Walker's index obscures lots of unrelated variation in the two series of enactments.

One other aspect of Table 2 is worth comment. A careful reader may have noticed that they recode their key variables from Table 1 prior to entering them into the analysis reported in their Table 2. Our original dependent variable is no longer the object of explanation in Table 2, so for this table, Sparks and Walniuk change their measure of early suffrage to one based on Walker's index; and they use a simple dichotomous (*yes/no*) measure for pressures from pro-woman-suffrage campaigns. Operationally, they also reintroduce my substantive emphasis on the civic power of women's voluntary groups. They note that state affiliates of the GFWC often endorsed woman suffrage as well as mothers' pensions, and so create a new variable (not used in Table 1) called SUFFRAGE ACTIVISM IN WOMEN'S GROUPS. This variable measures activities of a voluntary group, the GFWC affiliate in each state; and Sparks and Walniuk acknowledge that it is closely correlated with our variable about women's group endorsements. Their new SUFFRAGE ACTIVISM variable turns out to be "significant," as do their reworked measures of early suffrage and suffrage campaigns.

In their substantive story about Table 2, they arrive at a truly bizarre position. They acknowledge that members of state legislatures during the 1910s often faced clubwomen who were calling for both mothers' pensions and woman suffrage. Many legislatures enacted mothers' pensions (by overwhelming nonpartisan majorities) very soon after women's clubs started asking for them. But Sparks and Walniuk would have us believe that they did this not because legislators were persuaded that such social legislation was a good idea but instead because legislators anticipated female suffrage and/or feared eventual bloc voting by women. Legislators are supposed to have thought this way despite the fact that morally vivid arguments for mothers' pensions were mounted by both prosuffrage and antisuffrage women's groups. And legislators were supposed to have acted out of fear of bloc voting in states where actual voting by women was absent and often not in prospect for years to come. We can only marvel that Sparks and Walniuk's legislators of the 1910s were folks with remarkable—indeed, teleological—powers of foresight.

CONCLUSION

This sort of convoluted argument (in which voting or the anticipation of electoral results becomes the prime activity and sole motivation for citizens, women's group leaders, and legislators alike) is made throughout the Sparks-and-Walniuk piece. These authors simply cannot credit any reasoning on the part of politicians except fear of punishment at the polls—just as they cannot credit any kind of political influence by women or other societal actors except for the exercise of voting power or the quest to attain the suffrage.

I certainly do not claim that voting is insignificant. During the early twentieth century, organized American women sometimes achieved legislative goals in the face of strong opposition by *combining* widespread civic mobilization for agenda setting with (actual or potential) voting. This is what happened in the enactment of the minimum wage law for women workers in California in 1913 (see the discussion in Skocpol 1992, 415–16). This also happened in the enactment of the federal Sheppard–Towner Act in 1921 (see the discussion in Skocpol 1992, chap. 9). In a few states during the 1910s, mothers' pensions were also probably achieved through a combination of women's civic action and voting.

But as Figures 1 and 2 (along with the statistical results in Skocpol et. al. 1993) vividly reveal, the overall tempos and priorities of mothers' pensions enactments between 1911 and 1935 cannot be attributed to the actuality or near-future threat of women's voting. In most states, mothers' pensions were enacted by overwhelming majorities of state legislatures almost at once after women's federations endorsed them. Suffrage, actual or near-potential, was not necessary for organized women to achieve mothers' pensions. Sparks and Walniuk's attempt to prove otherwise cannot stand because their variables are improperly manipulated to fit the results they want.

Unlike Sparks and Walniuk, I do not like to engage in pure theoretical deduction about what "must have been" in the minds of state legislators when they voted for mothers' pensions. If I had to speculate, I would guess that legislators, along with many citizens across America in the early 1910s, heard the arguments put forward by clubwomen in the Congress of Mothers, the GFWC, and many other women's voluntary associations. Legislators and many citizens were convinced by arguments about the social worth of motherhood and about what was best for the children of widows. Mothers' pensions became "an idea whose time had come." Something similar happened with woman suffrage itself. When they voted for suffrage, legislators were certainly not responding to female bloc voting. Rather, they must have become convinced that the time was right for this change. And they certainly responded in part to civic mobilization by suffragists organized into widespread federations of voluntary groups.

My suppositions about why legislators responded to calls for reforms on behalf of women cannot be proven, for there is little systematic evidence about what was in the minds of state legislators back in the early twentieth century. My guesses do, nevertheless, correspond to the historical record and to what we can learn from models and statistical tests based on honestly measured variables. The same kinds of evidence also validate my arguments about the political achievements of ordinary American women. Millions of these women used civic mobilization to influence agendas for public discussion and legislation. They did this even before most of them had gained the full right to vote in the various states of these United States.

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Notes

Sparks and Walniuk would like to thank William Keech and Stephen Leonard for helpful comments on earlier versions of this paper.

1. An additional question is *why* these groups became interested in political issues so soon after federation at the national level. The majority of women who were founders of the GFWC were also active proponents of suffrage and had seen through their own clubs that the quickest way to win a reluctant woman over to the cause was to increase her awareness of the need for political change and then convince her to lobby for those changes. She would then be likely to discover that her demands had little influence with the legislators, because she lacked the force of the ballot. One suffragist referred to the state federations of clubs as "suffrage kindergartens" for the masses of apolitical women (Harper and Anthony 1902, 781). Clearly, suffrage advocates who founded the GFWC (e.g., May Wright Sewall, the first vice-president of the organization, and Harriet Hanson Robinson, member of the first board of directors) were aware of its potential as a tool for educating and motivating women to work for suffrage and it seems possible that this was a prime reason for their efforts to foster this sort of organization at the federal and state levels. (For information on GFWC founders, see Blair 1980; Campbell 1979; and Wood 1912. Information on their suffrage activities can be found in Harper 1922; Harper and Anthony 1902; James, James, and Boyer 1981; and Leonard 1915.)

2. The activity by women's clubs that Skocpol and her associates identify in equal suffrage states, however, was usually under the aegis of a state legislative council or good government league, which meant that the women were lobbying not merely as "informed mothers" but also as informed voters. Skocpol and her colleagues also credit the National Congress of Mothers (another women's organization of this era) with working for mothers' pensions. Although this group did not endorse suffrage at the national level until the federal amendment was all but assured, there is evidence of suffrage support among constituent clubs and several state affiliates endorsed suffrage between 1911 and 1918. Skocpol lists eight states (Idaho, Indiana, Iowa, Maryland, Maine, Mississippi, Oregon, Texas) where the Congress of Mothers was the only women's group to sponsor mothers' pensions (1992, 543–55). Five of the eight (Indiana, Maryland, Maine, Mississippi, Texas) were laggard states by Skocpol's definition, adopting after 1915. Women were already voting when mothers' pensions were adopted in two of the remaining three states (Idaho and Oregon). The Congress of Mothers was clearly, as Skocpol (1992) states, a strong and early proponent of the policy, but we would argue that its role in spreading the reform throughout the country was limited, given both its much smaller national membership (fifty thousand in 1911, compared to over one million for the GFWC) and the number of states in which the Mothers' Congress had branches (22 in 1911, compared to 48 for the GFWC).

3. In 1913 alone, there are at least 24 references to mothers' or "widows" pensions in the *Woman's Journal* and a similar number in 1915 and 1916. (The *Woman's Journal* editions of 1914 were not available.) In addition to several

endorsements of the policy from suffragists, social workers, and judges, there were also many articles that dealt specifically with the reform and how it was being applied in the various states. Several references to mothers' pensions can also be found in 1911 and 1912 editions, including an article by British suffragist, Sylvia Pankhurst, stating that these pensions were provided in many countries where women were allowed to vote.

4. Because state federations frequently reaffirmed their endorsement of suffrage each time there appeared to be a good chance of gaining the ballot, determining the exact year of endorsement is sometimes difficult. Also, since the membership and leadership within state federations changed over time, it is possible that at certain points, clubs that had previously endorsed suffrage were either no longer in favor of the reform or felt that it was best to keep the clubs nonpolitical despite their personal feelings on suffrage. Thus, the Appendix includes both the earliest endorsement of suffrage and the last endorsement prior to the enactment of mothers' pensions—as noted in the various state club records, Harper 1922, or editions of the *Woman's Journal*. It should be noted, however, that by 1912, the tide had definitely turned toward suffrage within the GFWC. The president of the organization, Eva Moore, cautioned against the GFWC's endorsing suffrage at the Biennial Convention of 1912 and narrowly managed to prevent a vote on the issue. Despite her own prosuffrage beliefs and those of most of the delegates to the convention, she felt that an official, national endorsement of suffrage would be counterproductive, because, as she stated, "it would take out of the organization the minority we want with us, the timid and conservative women from all over the United States who are working toward citizenship" (cited in O'Neill 1969b, 52). Many of the less timid and conservative members were angry that no vote was taken in 1912, and at least 15 state federations responded by endorsing suffrage (some for the first time) during the two years between national conventions.

5. For a discussion of "justice" and "expediency" as motivations in the suffrage campaigns, see Kraditor 1965. A central point of Kraditor's analysis of the suffrage movement is the supposed change in rhetoric that occurred within the movement after 1890, when the National Woman Suffrage Association and the American Woman Suffrage Association combined to form the National-American Woman Suffrage Association (NAWSA). According to Kraditor, the earlier movement had emphasized the "justice" issue, claiming that women should be granted the vote on the basis of their right to equal representation. After 1890, she asserts that the NAWSA shifted to arguments based on "expediency," arguing that women needed the vote so that they could better fulfill their roles as housekeepers and mothers. We are inclined to agree with Wheeler that both types of arguments were used on a fairly equal basis after 1890 (1993, 228). Indeed, given the strong justice arguments made by many leaders of the suffrage movement as late as 1920, it could be argued that the "expediency" language was merely a tactic to pull more women (and more conservative women) into the struggle for suffrage.

6. Iowa, Michigan, Nevada, New Jersey, Ohio, Pennsylvania, South Dakota, and Wisconsin. In Iowa, New Jersey, Pennsylvania, and Wisconsin, it was necessary for a second legislature to approve the measure. This approval was gained in all four of these states at the next regular session of the legislature. The referendum measure in Wisconsin was, however, vetoed by the governor on the grounds that the issue had been defeated at the polls in 1912. Wisconsin is a prime example of a state where the legislature appears to have been in favor of granting suffrage to its women but liquor interests exerted great pressure to keep them away from the ballot box due to fears that women would enact prohibition legislation. The antisuffrage efforts from this group were also strong in Michigan in 1912, when a woman suffrage amendment lost by .001% of the total vote, amid clear evidence of fraud. Since the state constitution did not allow recounts on amendments, the decision stood. Similar accounts of strong pressure and even outright fraud were reported in Nebraska (1913), Ohio (1912), North Dakota (1915), and West Virginia (1916).

7. See Table A-1 for further information on coding.

8. See Appendix and Table A-1 for further information on coding. Information on suffrage pressure was obtained primarily from state reports in the *History of Woman Suffrage* (Harper 1922). Supplemental and confirming information was obtained from Catt and Schuler 1923, Buechler 1986, O'Neill 1969a, various issues of *Woman's Journal* and *Woman Citizen* (two suffrage publications), and state House and Senate Journals.

9. The period 1911–13 is equal to six, 1914–15 to five, 1916–17 to four, 1918–20 to three, 1923–28 to two, and 1931–34 is equal to one. The final grouping of two states (South Carolina and Georgia) that did not enact this legislation prior to the Social Security Act of 1935 is equal to zero.

10. One might argue that the model presented by Skocpol and her associates already considers the control factors that account for state variation (e.g., industrialization and modernization) and rejects these as insignificant. However, they are never considered as control variables in the final model. In addition, when the dependent variable is coded in this fashion, it is extremely unlikely that all of the important control variables will be considered. Using a measure that captures the difference in innovation within each state minimizes this difficulty.

11. In order to verify the accuracy of our results, we ran a regression in which we replaced CLUB ENDORSEMENT with Skocpol and her associates' original variable, WOMEN'S GROUP ENDORSEMENT. The results were similar to those using the dichotomous version. Omitting WOMEN'S GROUP ENDORSEMENT from the difference in innovations model did not result in a significant increase in the sum of squared residuals ($p >$ .08).

12. Since the DIFFERENCE IN INNOVATION ON SUFFRAGE variable considers whether or not women had suffrage, the variable SUFFRAGE AT THE TIME OF MOTHERS' PENSIONS is not included in our final model.

13. Some GFWC affiliates seem to have been aware of the possibility that they would lose members as other organizations developed. Lemons notes that many GFWC clubs were strongly opposed to the creation of Leagues of Women Voters, due largely to a feeling that important functions of the Women's Clubs would be usurped by the new organization (1973, 54).

14. The issue of presidential-only suffrage is ignored by Sparks and Walniuk. They should not have coded states with this sort of suffrage as the equivalent of full-suffrage states, because legislators in those states did not face women voters. At the same time, however, the enactment of presidential-only suffrage is a good indicator of a prosuffrage climate in a state. And this phenomenon speaks in telling ways to Sparks and Walniuk's "pressure" thesis. Starting with Illinois in 1913, quite a few state legislatures enacted this kind of suffrage as a way to start enfranchising women without a constitutional amendment or a popular vote in a referendum (see Kraditor 1971, 3–4). Obviously, the legislators who instituted presidential-only suffrage were not under pressure from women voters or even from people in general in their states. These legislators seem to have been "out in front" on the issue of suffrage.

15. Although we did not report this result in Skocpol et. al. 1993, we found that early suffrage had a bit stronger predictive effect when we changed our dependent variable into a dichotomous one, referring to states that enacted mothers' pensions before or after 1915. Of course, this is a very crude dependent variable, one that suppresses most of the interesting variation about the timing of mothers' pension enactments. Moreover, in this test we did not pit early suffrage against women's voluntary group endorsements of mothers' pensions. The two occurred together, after all, in virtually all of the suffrage states that enacted mothers' pensions by 1915. The only exception is Illinois, where all historians agree that a juvenile-court judge (rather than women voters or voluntary groups) was the one that persuaded the legislature to enact mothers' pensions in 1911. Moreover, women in Illinois could vote only starting in 1913, and then only for presidential

electors, not for state legislators. The upshot of all of this is that analysis of very early suffrage states cannot differentiate between the effects of women's voluntary groups and women as voters.

16. Most political scientists assume that voting is a preeminent form of political influence, so they are not likely to look too critically at tables that purport to prove such an argument. At the same time, many feminists prefer to believe that equal suffrage was the most important women's reform of the early twentieth century. And some feminists find it distasteful to contemplate that nonvoting married women, most of whom were not in the paid labor force, could have exercised significant political influence through voluntary civic action. The activities of such women seem to many to be "outside of politics," just as Sparks and Walniuk suggest.

17. I do not believe that a more nuanced measure is as impossible as Sparks and Walniuk claim. They could, for example, have developed a variable referring to positive and negative outcomes on the dozens of suffrage amendment campaigns conducted across the states from the nineteenth century to the 1910s. Or Sparks and Walniuk could have supplemented their list of prosuffrage factors with such obvious "negatives" as defeats of votes in major state houses, defeats of popular referenda, and refusals of states to ratify the Nineteenth Amendment. I believe that they made little effort to incorporate negatives into their assessment of the "strength" of state suffrage campaigns, because their overriding desire was to get every state to level 2 on their list of positive indicators (for reasons I am about to explain).

18. I base this, for example, on the way Sparks and Walniuk treat Oklahoma in Table A-1. They assign one prosuffrage point each for endorsements of suffrage by the Republican and the Progressive parties in that state. This allows them to count Oklahoma as a strong suffrage campaign state; and that in turn allows them to multiply 1 for Oklahoma by 3, because Oklahoma enacted mothers' pensions in 1915. Notice that if they had used a strict standard for "major party," including only Democrats and Republicans, Oklahoma would have ended up with a score of 0 on their final SUFFRAGE CAMPAIGN variable. This state is one of several (see also the cases of Illinois, Minnesota, and New Jersey) where their willingness to count smaller-party endorsements of suffrage as one of two favorable factors makes all the difference between a high SUFFRAGE CAMPAIGN score (3 or 4) and a score of 0.

19. Sparks and Walniuk say that they constructed their new variables in the "same way" that my associates and I constructed our WOMEN'S GROUP ENDORSEMENTS variable. But they certainly misunderstood something if they think this. To be sure, our variable measured the timing of the endorsement(s), if any. But it was straightforwardly coded, without any reference at all to when—or whether—the state in question enacted mothers' pensions. To construct our variable, we scanned state reports to GFWC Biennal conventions *for the entire period of the 1910s and 1920s*. We also examined Mothers' Congress records through the mid-1920s, and further state-level sources whenever possible. For a complete accounting of our sources, see Skocpol (1992: App. 2). We did *not* only look at the years just prior to mothers' pension enactments; instead we made every effort (given available sources) to do a complete scan for the 1910s and 1920s. As for the scoring of the variable itself, we coded each state directly, according to whether and when women's groups first endorsed mothers' pensions. For states where women's groups endorsed pensions in 1912 or 1913, we assigned a score of 4; for endorsements in 1914 or 1915, we assigned a 3; for endorsements in 1916 or 1917, we assigned a 2; for endorsements in 1918 or 1919, we assigned a 1; and for the remaining states, where no endorsements were found in the 1910s or 1920s, we assigned a score of 0. That was it. We engaged in no further transformations. Our variable was directly and consistently measured across all of the states.

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NOTES FROM THE MANAGING EDITOR

FORTHCOMING IN DECEMBER 1995

The following articles, research notes, and controversies have been tentatively scheduled for publication in a forthcoming issue.

David Austen-Smith and Jeffrey S. Banks. "Information Aggregation, Rationality, and the Condorcet Jury Theorem."

Jonathan Bendor. "A Model of Muddling Through."

Sarah A. Binder. "The Partisan Basis of Procedural Choice: Allocating Parliamentary Rights in the House, 1789–1991."

Bruce Bueno de Mesquita and Randolph M. Siverson. "War and the Survival of Political Leaders: A Comparative Study of Regime Types and Political Accountability."

Avinash Dixit and John Londregan. "Redistributive Politics and Economic Efficiency."

Darrell Dobbs. "Family Matters: Aristotle's Appreciation of Women and the Plural Structure of Society."

Kathryn Firmin-Sellers. "The Politics of Property Rights."

Mark J. Gasiorowski. "Economic Crisis and Political Regime Change: An Event History Analysis."

Jeffrey Isaac. "A New Guarantee on Earth: Hannah Arendt on Human Dignity and the Politics of Human Rights."

Jonathan N. Katz and Brian R. Sala. "Legislative Organization and the Electoral Connection in a Partisan Era: The Effects of Australian Ballot Laws on House Committee Tenure."

Cathie Jo Martin. "Nature or Nurture? Sources of Firm Preference for National Health Reform."

Amy R. McCready. "The Ethical Individual: An Historical Alternative to Contemporary Conceptions of the Self."

Jongryn Mo. "The Choice of Domestic Bargaining Regime in Two-Level Games: The Use of Agent Veto."

Burt L. Monroe. "Fully Proportional Representation."

Brian M. Pollins. "Global Political Order, Economic Change, and Armed Conflict: Coevolving Systems and the Use of Force."

Larry M. Preston. "Theorizing Difference: Voices from the Margins."

Keith Topper. "Richard Rorty, Liberalism, and the Politics of Redescription."

Charles J. Pattie, Ronald J. Johnston, and Edward A. Fieldhouse. "Winning the Local Vote: The Effectiveness of Constituency Campaign Spending in Great Britain, 1983–1992." A research note.

Jacqueline Stevens; Rogers M. Smith. "Beyond Tocqueville, Please! The Multiple Confusion of the Mainstream." A controversy.

FUTURE SUBMISSIONS AND CORRESPONDENCE

The new Managing Editor of the *American Political Science Review*, Professor Ada W. Finifter, has now assumed her duties. Manuscripts to be considered for publication, as well as correspondence concerning manuscripts *currently or previously under review*, should be sent directly to Professor Ada W. Finifter, *American Political Science Review*, Department of Political Science, Michigan State University, 202 South Kedzie Hall, East Lansing, MI 48824-1032.

BOOK REVIEWS

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Book Review Contents

POLITICAL THEORY

Dasgupta, Partha. *An Inquiry into Well-Being and Destitution* .. Edward F. McClennen Robert L. Hood 738
Derrida, Jacques. *Specters of Marx: The State of the Debt, the Work of Mourning and the New International* .... Romand Coles 739
Disch, Lisa Jane. *Hannah Arendt and the Limits of Philosophy* . Suzanne Duvall Jacobitti 740
Gillespie, Michael Allen. *Nihilism Before Nietzsche* .... Karsten Harries 741
Harries, Karsten, and Christoph Jamme, eds. *Martin Heidegger: Politics, Art, and Technology* .... Leslie Paul Thiele 742
Haslett, David W. *Capitalism With Morality* .... Alan Carling 743
Hinich, Melvin J., and Michael Munger. *Ideology and the Theory of Political Choice* .... Keith T. Poole 744
Holyoak, Keith J., and Paul Thagard. *Mental Leaps: Analogy in Creative Thought* .... Francis A. Beer 745
Horton, John, and Susan Mendus, eds. *After MacIntyre: Critical Perspectives on the Work of Alasdair MacIntyre* .... Richard C. Sinopoli 746
Ingram, Attracta. *A Political Theory of Rights* .... Alan Gewirth 747
Kraus, Jody S. *The Limits of Hobbesian Contractarianism* .... Russell Hardin 748
Matthews, Richard K. *If Men Were Angels: James Madison and the Heartless Empire of Reason* .... Vincent Ostrom 749
Miliband, Ralph. *Socialism for a Sceptical Age* .... John E. Roemer 750
Raz, Joseph. *Ethics in the Public Domain: Essays in the Morality of Law and Politics* .... Eileen Bresnahan 751
Shain, Barry Alan. *The Myth of American Individualism: The Protestant Origins of American Political Thought* .... Adam B. Seligman 752
Steiner, David M. *Rethinking Democratic Education: The Politics of Reform* .... David Fott 753
Trachtenberg, Zev M. *Making Citizens: Rousseau's Political Theory of Culture* .... Emily Hauptmann 754
White, James Boyd. *Acts of Hope: Creating Authority in Literature, Law and Politics* .... John H. Schaar 755

AMERICAN POLITICS

Altman, Dennis. *Power and Community: Organizational and Cultural Responses to AIDS* .... Mark Blasius 756
Berg, John C. *Unequal Struggle: Class, Gender, Race, and Power in the U.S. Congress* .... Rebekah Herrick 757
Bianco, William T. *Trust: Representatives and Constituents* .... Thomas W. Gilligan 758

Blasius, Mark. *Gay and Lesbian Politics: Sexuality and the Emergence of a New Ethic* . . . . Ronald J. Hunt 759
Campbell, James E. *The Presidential Pulse of Congressional Elections* . . . . John R. Petrocik 760
Goldin, Claudia, and Gary D. Libecap, eds. *The Regulated Economy: A Historical Approach to Political Economy* . . . . Lawrence S. Rothenberg 760
Johnson, Robert David. *The Peace Progressives and American Foreign Relations* . . . . Martha L. Gibson 761
Johnson, Ronald N., and Gary D. Libecap. *The Federal Civil Service System and the Problem of Bureaucracy: The Economics and Politics of Institutional Change* . . . . Brian J. Cook 763
Lashley, Marilyn E., and Melanie Njeri Jackson. *African Americans and the New Policy Consensus: Retreat of the Liberal State?* . . . . Lawrence Bobo 764
McCann, Carole R. *Birth Control Politics in the United States, 1916–1945* . . . . Kristin Luker 764
Mitchell, William C., and Randy T. Simmons. *Beyond Politics: Markets, Welfare, and the Failure of Bureaucracy* . . . . William R. Keech 765
Nagel, Robert F. *Judicial Power and American Character: Censoring Ourselves in an Anxious Age* . . . . John Anthony Maltese 766
Nichols, David K. *The Myth of the Modern Presidency* . . . . Robert Scigliano 767
Rothenberg, Lawrence S. *Regulation, Organizations and Politics: Motor Freight Policy and the Interstate Commerce Commission* . . . . Roger G. Noll 768
Weed, Clyde P. *The Nemesis of Reform: The Republican Party During the New Deal* . . . . Colin Gordon 769
Wood, B. Dan, and Richard W. Waterman. *Bureaucratic Dynamics: The Role of Bureaucracy in a Democracy* . . . . Larry B. Hill 770
Zernicke, Paul Haskell. *Pitching the Presidency: How Presidents Depict the Office* . . . . Kathleen Hall Jamieson 771

COMPARATIVE POLITICS

Bell, David S., and Eric Shaw, eds. *Conflict and Cohesion in Western European Social Democratic Parties* . . . . Herbert Kitschelt 773
Bengelsdorf, Carollee. *The Problem of Democracy in Cuba: Between Vision and Reality* . . . . Linda Fuller 773
Brenner, Reuven. *Labyrinths of Prosperity: Economic Follies, Democratic Remedies* . . . . John T. Williams 774
Brysk, Alison. *The Politics of Human Rights in Argentina: Protest, Change, and Democratization* . . . . David Pion-Berlin 775
Butler, David, Andrew Adonis, and Tony Travers. *Failure in British Government: The Politics of the Poll Tax* . . . . Anthony Barker 776
Coppedge, Michael. *Strong Parties and Lame Ducks: Presidential Partyarchy and Factionalism in Venezuela* . . . . Gary Hoskin 777
Cothran, Dan A. *Political Stability and Democracy in Mexico: The "Perfect Dictatorship"?* . . . . Martin C. Needler 778
Escobar, Arturo. *Encountering Development: The Making and Unmaking of the Third World* . . . . Victor V. Magagna 778
Friedman, Edward, ed. *The Politics of Democratization: Generalizing East Asian Experiences* . . . . William L. Richter 780
Hart, Vivien. *Bound by Our Constitution: Women, Workers, and the Minimum Wage* . . . . Judith A. Baer 780
Ihonvbere, Julius O. *Nigeria: The Politics of Adjustment and Democracy* . . . . Otwin Marenin 781
Kowalewski, David, and Dean Hoover. *Dynamic Models of Conflict and Pacification: Dissenters, Officials, and Peacemakers* . . . . Marc V. Simon 782
Miles, William F.S. *Hausaland Divided: Colonialism and Independence in Nigeria and Niger* . . . . Michael O. Anda 783
Roy, Olivier. *The Failure of Political Islam* . . . . Leonardo A. Villalón 784
Soper, J. Christopher. *Evangelical Christianity in the United States and Great Britain: Religious Beliefs, Political Choices* . . . . Lisa Langenbach 785
Stotzky, Irwin P., ed. *Transition to Democracy in Latin America: The Role of the Judiciary* . . . . Thomas D. Lancaster 786
Tarrow, Sidney. *Power in Movement: Social Movements, Collective Action, and Politics* . . . . Anne N. Costain 787
Tétreault, Mary Ann, ed. *Women and Revolution in Africa, Asia, and the New World* . . . . T. David Mason 788

Whatmore, Sarah, Terry Marsden, and Philip Lowe, eds. *Gender and Rurality* .... Linda J. Seligmann 789
Widner, Jennifer A., ed. *Economic Change and Political Liberalization in Sub-Saharan Africa* .... Julius O. Ihonvbere 790
Wieviorka, Michel. *The Making of Terrorism* .... Cynthia L. Irvin 791

INTERNATIONAL RELATIONS

Avant, Deborah D. *Political Institutions and Military Change: Lessons from Peripheral Wars* .... Kimberly Marten Zisk 794
Beckman, Peter R., and Francine D'Amico, eds. *Women, Gender, and World Politics: Perspectives, Policies, and Prospects* .... V. Spike Peterson 795
Ben-Dor, Gabriel, and David B. Dewitt, eds. *Confidence Building Measures in the Middle East* .... Allen Lynch 796
Brecher, Michael. *Crisis in World Politics: Theory and Reality* .... Charles F. Hermann 796
Choucri, Nazli, ed. *Global Accord: Environmental Challenges and International Responses* .... Marc A. Levy 797
Duke, Simon. *The New European Security Disorder* .... Ole Waever 798
Eichengreen, Barry, and Jeffrey Frieden, eds. *The Political Economy of European Monetary Unification* .... Leon Hurwitz 800
Fetherston, A. B. *Towards a Theory of United Nations Peacekeeping* .... Paul F. Diehl 801
Gowa, Joanne. *Allies, Adversaries, and International Trade* .... Michael D. Ward 802
Haas, Michael. *Improving Human Rights* .... Bruce E. Moon 803
Hollingsworth, J. Rogers, Philippe C. Schmitter, and Wolfgang Streeck, eds. *Governing Capitalist Economies: Performance and Control of Economic Sectors* .... J. Nicholas Ziegler 804
Hopf, Ted. *Peripheral Visions: Deterrence Theory and American Foreign Policy in the Third World, 1965–1990* .... George H. Quester 805
Klein, Bradley S. *Strategic Studies and World Order* .... David Dessler 805
Lustgarten, Laurence, and Ian Leigh. *In from the Cold: National Security and Parliamentary Democracy* .... Andrew Kirby 806
Merritt, Richard L., Robert G. Muncaster, and Dina A. Zinnes, eds. *International Event Data Developments: DDIR Phase II* .... Donald A. Sylvan 807
Moens, Alexander, and Christopher Anstis, eds. *Disconcerted Europe: The Search for a New Security Architecture* .... Paul Sharp 808
Murphy, Craig N. *International Organization and Industrial Change: Global Governance since 1850* .... Giulio M. Gallarotti 809
Sobel, Andrew C. *Domestic Choices, International Markets: Dismantling National Barriers and Liberalizing Securities Markets* .... Jonathan D. Aronson 810
Spruyt, Hendrik. *The Sovereign State and Its Competitors: An Analysis of Systems Change* .... Charles Tilly 811
Thomson, Janice E. *Mercenaries, Pirates and Sovereigns: State-Building and Extraterritorial Violence in Early Modern Europe* .... John A. Hall 812
Weltman, John J. *World Politics and the Evolution of War* .... David W. Ziegler 813
Whitworth, Sandra. *Feminism and International Relations: Towards a Political Economy of Gender in Interstate and Non-Governmental Institutions* .... J. Ann Tickner 814
Zimmerman, William, and Harold K. Jacobson, eds. *Behavior, Culture, and Conflict in World Politics* .... T. Clifton Morgan 815

POLITICAL THEORY

An Inquiry into Well-Being and Destitution. By Partha Dasgupta. Oxford: Clarendon, 1993. 661p. $45.00.

> When in the closing passages of *The Grapes of Wrath* Rose of Sharon in an act of beatific compassion suckles the starving stranger, her thought isn't that his preferences should count. Rose's quite spontaneous action is directed at the stranger's needs. She sees no philosophical dilemma here.
>
> —Partha Dasgupta

An Inquiry into Well-Being and Destitution is an analytical and empirical inquiry into the causes, nature, and consequences of an extreme condition of *ill*-being, destitution. The empirical focus is primarily on rural communities in the underdeveloped countries of Asia and Africa. As the epigraph suggests, the author has a passionate concern for the plight of the destitute. But theoretical discourses on this subject, he insists, have not faced the phenomenon as it occurs in the world we have come to know. To remedy this, Dasgupta has written an extraordinary, overwhelming, complex, and at times exasperating book. There are 25 chapters (8 being more technical "supplemental" chapters), 545 pages of text, and 80 pages of references. Some of the chapters are decidedly philosophical; others are given over to the presentation of various aspects of contemporary economic theory (general equilibrium theory, welfare economics, bargaining theory, etc.). The book's more technical discussions are based on recent work in both game theory and social choice theory. But it also reports on a staggering quantity of empirically based medical and scientific findings regarding the origins, nature, and consequences of destitution and statistical data regarding the incidence of destitution among populations of the world in the last few decades. All of this, in turn, provides a setting for quite a number of policy conclusions, regarding what must be done publicly and privately if conditions are to improve. These are developed individually and carefully within the context of specific aspects of the economic theory of resource allocation. Unfortunately, this means they are distributed over hundreds of pages, and that makes it hard to arrive at an overall sense of what he is prepared to recommend.

Despite the focus on Asian and African agricultural destitution, the book is of considerable value for anyone involved in research on economics and public policy, theoretical or empirical. The more theoretical sections are applicable to a much wider range of issues, and the presentations in general are sufficiently compartmentalized that the work can be treated almost as a kind of encyclopedia of philosophical, statistical, economic, and policy issues concerning not only destitution but well-being, the environment, ecologically sustainable development, social customs and traditions, future generations, rights and welfare, the measurement of well-being, the internal structure of the peasant family (household) unit, gender discrimination, and fertility (to mention only a few). It is also a remarkable compendium of what economists have said—and failed to say—about the subject.

The book is perhaps even more important, however (from the perspective of political theory and political science), for what it offers by way of philosophical foundations for making the elimination of destitution a priority and how such a concern translates into policy recommendations for reducing the incidence of destitution in the world. Following in the tradition of Rawls, Dasgupta appeals to the concept of a hypothetical social contract under conditions of uncertainty as a heuristic device for establishing what public priorities should be regarding destitution. Despite his passionate concern, he does not (as the remark about Rose of Sharon might suggest) rest the argument on moral intuitions regarding either our duty to the disadvantaged or the intrinsic badness of destitution. To the contrary, his social contract argument turns on the notion that there are substantial instrumental reasons why a representative party to the social contract would want to avoid such a state. To be destitute is to be without things that are essential for the pursuit of a well-lived life, things that enable or empower us to pursue whatever goals we have. Consistent with the liberal tradition that is associated with such a contract argument, Dasgupta invokes an individualistic and pluralistic conception of ends. Correspondingly, the state's role is to be *limited* to providing the requisite background environment and other all-purpose means that will enable persons to pursue their own ends. Contrary to the libertarian tradition, however, Dasgupta argues that the state must do more than just guarantee the negative freedoms associated with a system of private property, the sanctity of contract, and various political and civil liberties; it must ensure certain positive freedoms as well. More specifically, it must ensure that citizens have access to adequate amounts of basic needs-goods: food and clothing, potable water, environmental resources or land, health care, shelter, legal aid, education facilities, general information, or wealth enough to secure these things for themselves.

How is such access to be assured? This goes to the heart of Dasgupta's social contract argument. The real social world into which the parties to the social contract are to be thrust could, in principle, be one or the other of two types. It could be a stationary social system, in which the background environment and the motivations of agents remain approximately constant. In this kind of setting, one can expect (by appeal to recent work in game theory) that kinship and other local community groups will emerge to deal with problems of destitution and that such group solutions will persist over time. Just what specific kinds of groups will emerge is path-dependent; but these solutions will be stable in virtue of constituting coordination equilibria. Alternatively, persons may confront a dynamic situation in which the background environment and human motivations are subject to change, and in which there are no stable conventions.

The contemporary scene conforms to the second pattern. It is characterized by a dynamic environment, in which widening market opportunities, technological change, and exposure to new ways of living continually alter people's utility functions and motivations in ways that make kinship and group solidarity much more fragile. Citizens today, then, will insist on a social contract for such a dynamic uncertain world. Specifically, they will opt for a basic structure that does not rely upon (unstable) kinship and community groups as insurance against destitution but makes the state itself the guarantor of adequate levels of needs-goods. Thus, reflective, reasoned choice ratifies, in effect, what the global impact of technological, informational, and economic development has accelerated—the breakdown of social networks of family, kinship, and community, provisioning against destitution.

By way of addressing the question of what counts as an adequate level of needs-goods, Dasgupta explores at great length what is known about nutritional needs, malnutrition, undernourishment, (physical) maintenance requirements, and work capacity. The theme to which he repeatedly returns is that there is a level below which, no matter how hard one tries, one does not have the physiological abilities and energy to succeed. On his account, to be destitute is to be caught in a downward spiral from which one cannot extricate oneself. It is simply false, he insists, that those who own no physical assets still have one asset, namely, labour power. They have only potential labour power, and this can be converted into actual labour power only when they have access to adequate nutrition and health care. A point that should not go unnoticed is Dasgupta's emphasis on the importance of environmental goods for human well-being. Just as he recognizes physiological constraints on humans, he also recognizes ecosystem constraints on the sustainable use of natural resources. He also seeks to trace out a variety of links between nutrition, productivity, and involuntary unemployment.

What are the policy implications of all of this? In very broad terms, the message is that while there are certain things that must be left to the private realm, the economic theory of alternative resource allocation mechanisms itself implies that there is much that the governments of poor societies can and must do. The public realm is charged with the production and allocation of collective goods, which, for Dasgupta, turn out to include infrastructure, classical public goods (e.g., security, laws of property, and contracts), public health, common property resources, primary health care, school education, and food security (with the latter to be realized by means of targeted subsidies, food-or-cash for work, agrarian reform, and credit facilities). But as a contemporary economist, all of his recommendations are tempered with cautionary observations regarding the problems of adverse selection and moral hazard. Free-market economists, however, are likely to take exception to many of his conclusions, despite his careful qualifications.

More generally, the book is characterized by an unusual intellectual modesty. Repeatedly, he stops to observe that there is much we have yet to learn about both the physiology and the economics of destitution. Indeed, that tone is betrayed only by an indignant impatience with those who pursue problems in philosophical or economic theory that are divorced from the real world.

The thesis regarding what must be included in the social contract and its implications for public policy are very important. At a time when, in the United States, the political climate has turned sharply in favor of policies that are designed to decrease government responsibility, the message is that there must be greater government involvement in underdeveloped agricultural countries. There need be no inconsistency here. One could argue that devolution is both feasible and desirable once societies have achieved a significant level of affluence. But recent evidence regarding the distribution of wealth in this country (esp. the increase in the number of those who fall below the poverty line), the plight of the very poor with regard to health and nutrition, and the virtual collapse of community and kinship groups within the inner cities suggests a quite different conclusion. On our reading, Dasgupta's arguments yield implications for public policy in our own society that conflict sharply with those now in fashion. Indeed, at a time when intelligent discussion of public policy is in danger of being drowned out by ideological pronouncements and pop theories, thoughtful persons are likely to wish that there were more books like this and that Dasgupta would now turn his attention to the problem of destitution in an economically more advanced society, such as ours.

Bowling Green State University EDWARD F. MCCLENNEN
ROBERT L. HOOD

Specters of Marx: The State of the Debt, the Work of Mourning, and the New International. By Jacques Derrida. Trans. Peggy Kamuf. New York: Routledge, 1994. 198p. $55.00 cloth, $16.95 paper.

This book developed from a lecture Derrida first presented in 1993. It is quite poetic, moving from the breathless to the patient and painstaking, from the dirge to insistent moral exhortation, from the sermon to the giddy and vertiginous. Thematically, it is dense and far-ranging, shifting in and out of discussions of presence-absence, temporality, otherness, specters, ethics, politics, Third World debt, a "new international," and so forth.

Derrida situates his engagement with Marx in the context of the end of the Cold War amid increasingly deafening proclamations of the neoliberal "end of history." In a period of cynical political-economic hegemony concealed and proliferated by political, media, and academic discourses that are now "more than ever welded together," he seeks to provoke an opening by inheriting of "a certain spirit of Marx" (p. 53). Derrida distinguishes *this* spirit from "Marxism as ontology, philosophical or metaphysical system, . . . historical materialism or method, and from Marxism incorporated in the apparatuses of party, State, or workers' International" (p. 68). Aside from relatively vague references to the need to maintain the Marxist "problematic if not the conclusions" regarding questions of the market, state, and foreign debt, what most draws Derrida to Marx are the moments in the latter's writing that evoke a sense of radical openness in history, transformative possibility, the coming of the other, justice that exceeds the mere calculations of extant law and calls us endlessly to a responsibility toward the elusive particularities of the other: "We believe that this messianic remains an *ineffaceable* mark—a mark one neither can nor should efface—of Marx's legacy" (p. 28). Derrida distinguishes the messianic from religious and secular messian*isms*, which close this opening of experience to the other by giving it an exhaustive determinate content. The messianic permanently refuses to take what appears to be present as totally definitive; it thinks of history always in terms of the other and event *to come*. Marx covered over this opening with an ontological and eschatological "response"; and such closure was important in the political disasters of Marxism.

In contrast, Derrida calls us to the opening of this universalistic messianic structure as it works within practices concerning "the gift," justice, democracy, and responsibility—all of which must be understood as "coming" lest we fall into a closed complacency that cancels the possibility of the very thing whose presence we celebrate. He states that this position stems not from "a taste for the void or for destruction" but is rather a matter of "an ethical and political imperative" (p. 30), seemingly entwined with the problematic of "learning how to live," which begins *Specters of Marx*. Ethically and politically, he calls for a "complex and constantly re-evaluated strategy" of historical opening that would intertwine two different approaches: an immanent critique in which we strive "to adjust 'reality' to the 'ideal' in the course of a necessarily infinite process" and a farther-ranging questioning that would seek continually to problematize (move beyond, reformulate) specific ideals themselves insofar as they always fall short of our responsibility to the other. Without this tensional double strategy, Derrida warns us, we will return to "the worst" —"to a sort of fatalist idealism or abstract and dogmatic eschatology in the face of the world's evil" (pp. 86–87).

Concerning institutions, Derrida calls us to maintain a similar tension between remembering preservation and new transfigurations. The responsibility to which a certain *spirit* of Marx calls us should not be contained simply within a new scholarly project but "produce events, new effective forms of action, practice, organization" (p. 89).

However, as Derrida calls for this tension between openings to radical alterity and the necessity of more determinate theory and practice, he offers precious little in the way of the latter two. The politics of the "new international," while suggestive, remains extremely ephemeral. Might it not be developed in part through a textured critical engagement with the *substance* of the more admirable moments in Western Marxism? I also wonder whether self-critical reflections on the political implications of certain aspects of deconstruction might not have benefitted from a more determinate engagement with his primary interlocutor in *Specters of Marx*. In spite of these and some other reservations, I think *Specters of Marx* has many moments of brilliance. Derrida continues to extend and deepen his ethical and political reflections, and he has raised the volume of these to a level that ought to be audible now even to his deafest critics.

Duke University ROMAND COLES

Hannah Arendt and the Limits of Philosophy. By Lisa Jane Disch. Ithaca: Cornell University Press. 226p. $29.95.

Hannah Arendt has been the subject of many books and articles in the past few years. Lisa Disch's contribution to this literature is one of the best. Without minimizing the difficulties and ambiguities in Arendt's work, Disch exposes some of the most serious misinterpretations while showing how Arendt's work contributes to contemporary issues of democratic decision making, the politics of identity and difference, and political judgment. Disch's knowledge of Arendt is deep; and she draws on many unpublished, less familiar works as well as the standard ones.

Disch defines her purpose quite precisely. She seeks to use Arendt's work to resolve an "impasse" between neohumanist critical theorists (e.g., Jürgen Habermas, Seyla Benhabib) and poststructuralists (Michel Foucault, Bonnie Honig) about the possibility of reaching valid, impartial judgments in politics in a world where traditional norms of validity have collapsed. Neohumanists seek a democratic decision process in which, under conditions of mutual respect and equality, consensus can be achieved. Poststructuralists fear that consensus will always have the effect of denying and suppressing difference; the best we can do is embrace the fragmentation of local perspectives. Each side has attempted to appropriate Hannah Arendt's work as supportive of its point of view. Disch maintains that Arendt belongs to neither side but that her work can be used to resolve the impasse between them.

Disch describes Arendt's dissatisfaction with traditional philosophical and historical methods, which assumed that knowledge must be achieved from an "Archimedean point"—a detached standpoint permitting absolute objectivity. The notion of an Archimedean point is defective, Arendt believed, for two reasons. First, claims to absolute knowledge translate too readily into claims of power. Archimedes himself sought a lever with which to move the world; and this model of knowledge led Plato and the Western tradition to separate those who know (and hence should rule) from those who obey. Here, Arendt anticipated postmodern arguments of power/knowledge. Second, the Archimedean ideal of knowledge assumes that the only way to achieve impartiality and objectivity is to abstract oneself from all particular situations and involvements, a position that tends to suppress all difference. Here, Arendt anticipated contemporary arguments by feminists and others about identity and difference.

Arendt's alternative to Archimedean thinking, Disch argues, is *storytelling* (the use of particular narratives about particular characters and situations, as well as other literary devices) as a way of achieving a kind of *situated impartiality* (p. 164). Arendt used such techniques before they became fashionable and—because she did not explain her methods—was much misunderstood. One of Disch's contributions is to "tell a story Arendt did not tell," a story of how Arendt's methods of storytelling developed (p. 106).

Arendt began to practice these methods in the 1940s as she discovered the inadequacy of traditional categories of thought for understanding totalitarianism and judging its unprecedented evils. Misunderstandings of *Origins of Totalitarianism* forced her to reflect on her methods and defend them. What emerged, Disch asserts, was a form of "situated critical thinking"—thinking that remains engaged in a situation yet seeks a more impartial, critical understanding by attempting to comprehend and represent the very different perspectives of different participants (p. 110). In a sense, Disch suggests, Arendt tried to do for totalitarianism what Thucydides did for the Peloponnesian Wars when he sought impartiality by presenting speeches representing diverse viewpoints.

Late in her life, Arendt tried to justify such an approach to thinking, using the idea of enlarged mentality that Kant developed in his critique of judgment. Arendt interpreted Kant as proposing that one train "the imagination to go visiting" so that one attempts in imagination to tell stories about a situation from as many different perspectives as possible (p. 158). Arendt did not claim that a definitive judgment can be reached in this way but that this approach can teach others to think and judge for themselves. This approach is congenial, Disch argues, to what Arendt called, in her writings on Jewish identity, the "conscious pariah"—one who seeks neither assimilation into, nor escape from, society, one who can therefore find a stance of situated impartiality from which to engage in storytelling. Disch also uses Arendt's correspondence with Karl Jaspers to shed light on Arendt's struggles with her own Jewish–German identity and to provide a model of intellectual friendship in which differences are articulated and respected.

Disch praises Arendt for anticipating many of the insights of twentieth-century feminists and critics of modernity. She faults Arendt for slipping back at times into traditional modes of thought; but rather than focus on faults, she chooses to interpret Arendt's work "through the frame of its most radical insights" (p. 25). Building on these insights, she recommends that we adopt the practice of including dissenting opinions in the record of political debate, so that plurality is not eliminated after the fact in misleading claims of consensus.

This is not a book for the novice. Disch assumes her readers are familiar with Arendt's work and with current philosophical debates between humanist critical theorists and deconstructionists. To such readers Disch brings her rich, understanding of Arendt's published and unpublished writings. But these debates take place among a small segment of political theorists interested in continental European thought and radical politics. This is certainly the location of much of the recent scholarly interest in Arendt; and Disch locates herself quite intelligently in the context of that literature. But in doing so, she risks unduly limiting her audience and minimizes the importance of her work.

The problems Disch addresses face everyone today—conservative and radical, liberal and communitarian—insofar as they are interested in peaceful resolution of problems in a pluralistic society. At the theoretical level, how can we engage in situated thinking without metaphysical grounding yet attain some degree of detachment or objectivity? And at the practical level, how can we—coming from different cultures, genders, classes, and so on and with no shared standards to appeal to—communicate with each other and reach decisions that have some claim to validity? If Disch is correct that Arendt can help us think about these problems (and I am convinced that she is) then many people, not just radical intellectuals engaged with European thought, should read her book and read Arendt under its influence.

Southern Illinois University, Edwardsville SUZANNE DUVALL JACOBITTI

Nihilism before Nietzsche. By Michael Allen Gillespie. Chicago: University of Chicago Press, 1995. 311p. $27.50.

If the title of this thought-provoking book promises a study of nihilism before Nietzsche, its goal is rather more ambitious—to show that the history of modern thought has been "the ever more explicit revelation of the foundation of modern reason in will" (p. 256)—in a way that invites comparison with Hans Blumenberg's *The Legitimacy of the Modern Age* and *The Genesis of the Copernican World*. Gillespie, too, interprets modernity as a response to the breakdown of the never quite seamless scholastic synthesis of reason and revelation. But while Blumenberg understands modernity as the second and now definitive overcoming of gnosticism (a thesis presupposing that Christianity's first overcoming of it at the beginning of the Middle Ages was unsuccessful), for Gillespie, the problem faced by modernity (and more especially by Descartes) is "not a Manichean gnosticism that is opposed to God, but a skepticism that arises out of divine omnipotence" (p. 49). Behind Descartes's evil genius lurks the incomprehensible God of the nominalists; and though modernity was constructed to constrain this "dark God" (p. 257), he returned in the modern notion of will, which Gillespie would have us understand as a secularization of divine omnipotence, thus calling into question Blumenberg's rejection of the secularization hypothesis.

The key figures in this challenging construction of the genesis of the modern world are Ockham, Descartes, and Fichte. Supported by Black Death and the papal schism, the God of the nominalists, "a terrifying, transrational, transnatural God of will" (p. 255), is said to have brought the medieval world to an end and to have left human beings adrift in an infinite and unfathomable universe with no guarantee of happiness in either this world or the next.

Gillespie recognizes that the rise of the new science would have been impossible without a renewed confidence in man's ability to comprehend nature. In this connection, Blumenberg had pointed to humanism as one of the conditions that made a Copernicus possible. Gillespie's darker narrative skips over humanism, as it skips over the humanist side of Descartes. Thus while Gillespie grants that Descartes "constructed his bastion of reason to shield man from [the nominalist God of will] and to establish a certain and secure citadel from which to undertake the conquest of the natural world," such construction is said to have required Descartes to attribute "to man the same infinite will that had proven so problematic in God" (pp. 253, 255). But this interpretation of Descartes invites challenge, as Gillespie seems to recognize when he admits that "this notion of will remained implicit in Descartes's thought" (p. 255). Does the Cartesian project really presuppose that the human being is capable of occupying the place of Ockham's dark God? Such a reoccupation would indeed have to undermine "the basis for reason that Descartes sought to establish" (p. 62). But it fails to do justice to what we can call *Cartesian faith*. A careful reading of Descartes offers little support to the claim that modern reason has its foundation in will. To be sure, Descartes recognizes the infinity of the human will and its power to lead us into error and sin, but he also knows that the pursuit of truth demands the submission of the will to finite human reason. The refusal to accept the limits set to his power has to render the human being measureless and abysmal. To the extent that freedom is divorced from reason, we can indeed speak of a return of gnosticism and of a need to renew the battle against it.

But in this battle Descartes would be an ally, not an enemy. With his insistence on the limits of human reason, he is much closer to Kant than to Fichte, whose idea of the absolute is understood by Gillespie as the crucial step that (as Jacobi recognized) must lead to nihilism. In important chapters, Gillespie points out how romanticism, with its celebration of such self-centered, demonic heroes as Byron's Manfred, Goethe's Faust, and Tieck's William Lovell, unfolded the implications of Fichte's idealism, as did the "Left Hegelian misappropriation of Hegel's principle of negation" (p. 127) and Russian nihilism's celebrations of Prometheus. More questionable are the chapters that insist that Schopenhauer, too, with his metaphysics of will, and (following him even while resisting his pessimism) Nietzsche remain caught within Fichte's orbit, bound by his notion of will, which "lies at the heart of nihilism" (p. 200).

Any broadly sketched narrative of this sort has to invite challenge, both by the historian of ideas and by the philosopher. The historian of ideas will question the leaps in this narrative, especially the overleaping of science. Thus, Copernicus is not mentioned at all, Galileo, only briefly in the Descartes chapter. Could one not argue that modern science is nihilistic in its very essence, not because it is supported by a will that should be understood as a secularization of the nominalist God but because of the way reason has so limited itself and reality that in this reality, no place is left for either values or God. Nietzsche's placement of Copernicus at the beginning of a still-progressing diminution of man and a still-growing nihilism deserves more careful consideration: Does it not provide a better key to the nihilism of the modern age than Gillespie's appeal to an unfettered will that invites us to consider it as a secularization of Ockham's God? Does that reason which presides over modern science not lead to an ever more profound sense of homelessness, does it not invite the divorce of reason and will and thus a return of gnosticism?

The linking of nihilism to science and to its progressive diminution of man need not contradict claims that nihilism grows out of the sin of pride or its secularized counterpart. Did not the snake's promise lead to a diminution of man? "The possibility of coming to terms with modernity, or passing beyond it" depends, I would suggest, not so much on our ability to face the question posed by the supposed reemergence of the "dark God that modernity was constructed to constrain" (pp. 256–57) as on our ability to understand not just the legitimacy of modern science and its governing reason but also its limits on our ability to put science in its proper place.

Yale University KARSTEN HARRIES

Martin Heidegger: Politics, Art, and Technology. Edited by Karsten Harries and Christoph Jamme. New York: Holmes & Meier, 1994. 271p. $34.95.

Each of the 16 essays of this fine collection offers something of interest. Rather than provide summaries, however, I shall focus on the volume's central theme of the relation between philosophy and practice.

Heidegger's *Beiträge zur Philosophie* is a chief concern. The *Beiträge* (vol. 65 of the *Gesamtausgabe*) was published in 1989, the centenary of Heidegger's birth and the year of the colloquium held at Yale University where the essays of this volume were first presented. Written by Heidegger in the years 1936–38, the *Beiträge* is considered by many to constitute Heidegger's second major work, after *Being and Time*.

The allure of the *Beiträge* is heightened by its potential to save Heidegger from those critics who condemn him for his allegiance to National Socialism. Klemens von Klemperer reminds us that unlike Oswald Spengler and Ernst Jünger, who lent philosophic momentum to nazism but refused an official connection, Heidegger, like Carl Schmitt, actually joined the Nazi party. Heidegger was more committed to the movement than to the party itself, and he would quickly find himself at odds with party dictates. Nonetheless, Heidegger philosophically propagandized for the party while serving as rector of Freiburg University in 1933–34. Klemperer observes that Heidegger's grievous mistake here was to misconstrue his role as a philosopher. By rights, the philosopher is a seeker, and Heidegger, with justification, has been called the "greatest seeker of this century" (p. 15). Yet (for a time anyway) Heidegger also wanted to be a finder—indeed, even a political enforcer of his findings.

Did Heidegger remain committed to National Socialism after the rectorate? This question was famously revisited and answered in the affirmative by Victor Farias in *Heidegger and Nazism* (Eng. trans., 1989). The *Beiträge*, according to Alexander Schwan, supplies "a single, great refutation" of Farias's claims (p. 79). Likewise, Reiner Schürmann insists that the *Beiträge* demonstrates Heidegger's achievement of a "tragic knowledge" that militates against any totalizing force that would hubristically suppress concrete particularity. A number of other contributors make the point that the *Beiträge's* criticism of the totalizing aspects of "machination"—its "using up" of the world and its inhabitants—effectively targets Nazism.

If the *Beiträge* exhibits Heidegger's renunciation of nazism, it is a small consolation. The muted rejection of nazism is placed in the context of a simultaneous rejection of liberalism and democracy, which are found equally complicitous in modernity's project of forgetting Being. Moreover, as Schwan observes, Heidegger's all-or-nothing relation to modernity and metaphysics demonstrates the same disregard of heterogeneity—the same monolithic totality—that is decried in Heidegger's criticisms of technological enframing (p. 76). In turn, the *Beiträge*, like the rest of Heidegger's work, is bereft of even the slightest admission of political responsibility or moral guilt. Finally, Heidegger's renunciation of nazism, Schwan reminds us, did not lead to an active resistance but rather to a "turning inward," to a renunciation of all political practice and responsibility (p. 86). This mugwumpery, one might suggest, betrays the same intellectual disdain for the world of political give-and-take that originally led Heidegger to embrace a radical, revolutionary force that might, in Schwan's felicitous phrase, "hurry along the event of Being" (p. 84).

In the same vein, Karsten Harries contends that "Heidegger's ontological insight rendered him ontically blind" (p. 237). The task for Heidegger's readers, then, is to benefit from his ontology without similarly losing a sense of responsibility for the world. Without this ontic grounding, Harries suggests, such Heideggerian notions as authenticity, resoluteness, and historicity become, as Herbert Marcuse once said, "empty sails . . . at the mercy of the prevailing winds" (p. xx). Harries also makes the Derridean point that Heidegger's philosophy prompts us to think what Heidegger the man could not think, namely, the thought of the Holocaust from the point of view of the conditions of modernity that made it possible.

In the discussion section, Richard Rorty takes issue with Harries's contention that we need to think more deeply about the Holocaust and National Socialism. For Rorty, the rise of nazism and its final solution should be understood simply as an example of "thugs" winning out over liberal democrats. We should not try, as Heidegger himself did, to establish any "internal connections" between actual political movements and supposedly world-historical forces like technology, metaphysics, or modernity (p. 248). German history does not hold a special philosophic lesson, Rorty claims. The fascist thugs in Germany simply had "better luck" than their counterparts did in France, England, and America (p. 250).

Likewise, Rorty insists that Heidegger's work is not inherently fascistic. Rorty portrays "another possible world" in which Heidegger marries a Jew rather than Elfride Petri and flees nazism to America rather than becoming rector at Freiburg, but writes virtually the same philosophic works as he actually did. Fate, in other words, plays as large a role in history for Rorty as it does for Heidegger. The difference is that for Rorty, fate is raw, undiluted happenstance. For Heidegger, fate is often understood as destiny; and destiny is often understood in terms of a national calling to greatness.

The point, for Rorty, is not to be caught up with essentialism. He views Heidegger's writings as a toolbox from which scavenging philosophers might choose things useful to their projects. He suggests that we "read Heidegger's books as he least wanted them read"—that is, pragmatically, as "standing-reserve" awaiting exploitation in the service of liberal democracy (pp. 35, 40).

The desire to make political use of philosophy, Thomas McCarthy points out in his essay, was also what prompted Herbert Marcuse to distance himself from Heidegger. Marcuse was an avid Heideggerian for some time before turning to Dilthey and Marx in an effort to link philosophy to political practice. Subsequently, Marcuse would disavow his mentor altogether, following Heidegger's infamous "silence" after the war.

Extruding political ramifications from Heidegger's philosophy is certainly no easy affair. One largely unexplored avenue concerns Heidegger's notion of freedom. That Freedom is a central feature of Heidegger's philosophy one learns from Joachim Storck's account of Heidegger's lifelong correspondence with Elisabeth Blochmann, a half-Jew whom Heidegger met through his wife in 1917. Philosophy, Heidegger explains to Blochmann, "has its own faith—which is the freedom of [human] existence itself, which only comes to exist in *being* free" (p. 47). Randall Havas and Alexander Schwan also pay attention to Heidegger's concern with freedom. However, none accounts for Heidegger's radical transformation of the concept, a transformation that suggests important practical consequences in the fields of politics, technology, ecology, and art.

Linking Heidegger's concern with art to his politics, Karlheinz Stierle contends that Heidegger's "radical" understanding of art led him to an equally "radical political engagement" (p. 157). This linkage is revisited in the concluding discussion when an unidentified questioner asks the contributors whether nazism should be understood as a fundamentally totalitarian venture corresponding to a hyperrationalized metaphysics or as a fundamentally fascistic venture corresponding to an irrational, aesthetic mood. The question is mostly left unanswered. Yet I believe (pace Rorty) that it goes to the heart of Heidegger's political "error."

Heidegger effectively reinstated metaphysics in his aesthetic appeal to an immanent national destiny. The colleague who greeted Heidegger after his resignation of the rectorate with the sarcastic remark, "Back from Syracuse, Herr Heidegger!" hit the nail on the head. If Platonism may be defined by its effort to incarnate metaphysical ideals in the political world through a masterful, representative artistry, then Heidegger's effort at Freiburg remains eminently Platonic in its metaphysical understanding of political art.

Heidegger's faith in the Führer's capacity to concretize the folk's destiny did not last much beyond the rectorate. Years later, however, Heidegger retained the hope that the Germans might still fulfill their unique historic mission. Despite veiled recantations in the *Beiträge*, one suspects that Heidegger never found the folkish idol completely hollow. The contributors to this volume are uneasy with this side of Heidegger, as they should be. Yet they seriously and insightfully engage Heidegger's philosophic achievements nonetheless, and we are the better for it.

University of Florida LESLIE PAUL THIELE

Capitalism with Morality. By David W. Haslett. Oxford: Clarendon, 1994. 280p. $49.95.

This is a bold and a worthy book. Not for Haslett the painful clamber up the rock-strewn gully toward the summit; rather, the leap from crag to crag. His range is wide, and his footing mostly sure. He first defends his political philosophy (a variety of rule utilitarianism) against the skeptics and the libertarians and then applies it to a number of contending social systems, winnowing their features to come up with a welfare-maximizing version of what he still calls *capitalism.* This winnowing is achieved with a nice disregard for stereotypical clusters of positions; one might say that in Haslett's utopia there is something for almost everyone. He is against central planning but for workers' control, in favour of the market but against inequality, against minimum wages but for income credit, against government interference but in favour of universal welfare provision, for capitalism but against the inheritance of property, against Marxism but in favour of the Marxist agenda. The only social character he thoroughly disapproves is one who is, by choice, unmotivated by the work ethic (p. 201). Haslett can be impatient, too, with those who do not see things quite his way. Those who have spent their working lives puzzling over the holism/individualism problem might feel disheartened to be told not to "waste . . . time with such nonsense" (p. 96). But with so much virgin territory to cover, it is perhaps inevitable that Haslett will sometimes eschew the scalpel for the axe.

He proposes a "compromise" utilitarianism (actually a synthesis of preferential and experiential versions) that measures well-being by the degree of preference satisfaction people enjoy in respect of the preferences they have over their experiences, the best social system being the one that arranges its institutions to maximize the equal well-being of its citizens in this sense (pp. 27–33).

Now, Haslett argues persuasively that in the business of weighing up social systems, it is as well to take account not only of the desirability of institutions but "the social pressure system" that will be required in practice to ensure the adherence to institutional norms (p. 11). This is a welcome precaution against excessive idealism (it is presented as a necessary concession to human fallibility), but it creates difficulties for other parts of Haslett's analysis.

First, in order to guard the preference formation process against the effects of "irrational impulses, superstitions, prejudices and the like," agents are assumed to possess perfect insight into the experiences of which they are to form a preference (p. 29). Second, the attention to norm compliance embeds the norms in the context of a social system, and it is the complete working model of this system that is now subject to comparative evaluation. We might then wonder, if this—I dare say it—holist impulse were pushed too far, whether it might not call for an historical (or at least a sociological) analysis of what makes social systems hang together or might not imply the creation of distinctive personalities or preference structures in the populations of two social systems that would vitiate the basis for welfare comparisons between them, on the grounds that the well-being of different folks is served by different strokes.

Forging ahead, Haslett nevertheless argues that the four values that conduce most to the overall well-being of his archetypal Any person are (1) equality of access to the goods and services necessary for a decent life, (2) equal opportunity, (3) freedom, and (4) economic productivity (p. 42). He unsurprisingly finds actually existing capitalism wanting in respect of values 1 and 2 and central-planning socialism wanting in respect of values 3 and 4. The heart of the book therefore consists in the attempt to construct in the mind's eye a social system that combines its features in order to maximize the joint realization of values 1 and 4. Markets are in because central planning is out; but worker control (i.e., workplace democracy) is also in, because it is much better than corporate control on values 1–3, without being much different on value 4. Haslett prefers a breed of workers' control with decentralized investment decisions, which he calls *worker-control capitalism*—a slightly strange locution, given that means of production are collectively owned and controlled by (distinct) groups of workers and that it is "the closest we can come to a classless society" (p. 189). But the classless mode of production also needs backing by antipoverty measures, public provision of child care, and state funding of educational, medical, and legal expenses. There is a thorough discussion of the merits of earned income credit in alleviating poverty without damaging incentives, although a comparison with an unconditional basic income (the leading proposal in this field in Europe) would have been welcome. I was less impressed by the discussion of health (and, to some extent, higher education), because systems better in respect of values 1 and 2 than any Haslett proposes and not obviously inferior on values 3 and 4 are currently being destroyed before our eyes in Britain. But he returns to form near the end, with a no-nonsense demonstration that capitalist principles demand the abolition of inheritance laws, coupled with an elegant proposal to limit not bequests themselves but individual receipts from inheritance.

I hesitate to enter any criticism derived from a man and works cast aside so briskly (p. 188), but two points of Marxian provenance did strike me. First, Haslett speaks of economic productivity as if it includes the intensity and duration of work, so that enhancing incentives ipso facto makes the economy more productive by inducing workers to work longer and/or harder. But why should productivity in this sense enhance well-being? I would have thought, if anything, the reverse. The appropriate productivity value is surely labour efficiency—being output per labour input—as the Marxists contend. Second, Haslett at various points seems to underestimate the problems of transition to a morally defensible capitalism, which are largely the problems of unmoral capitalism's vested interests (e.g., pp. vii, 133, 154, 166, 189). I am not sure that even in the long run, the best argument will always combine with political democracy to create the social system that is in the best interests of the majority, even supposing we can agree what that might be. Rather like libertarianism itself and despite Haslett's admirable efforts, bad practices can sometimes persist for less-than-intellectual reasons.

University of Bradford ALAN CARLING

Ideology and the Theory of Political Choice. By Melvin J. Hinich and Michael Munger. Ann Arbor: University of Michigan Press, 1994. $42.50.

This is an ambitious book. Hinich and Munger argue that formal political theory is at a "critical juncture, where internal contradictions and the lack of interesting or useful new results have set members of the discipline to casting about for alternatives" (p. ix). The crux of their argument is that formal theory begins at the wrong place. Political choices are not like choices made in a market, and a narrow focus on individual self-interest will fail to explain human action in politics. Ideology, culture, moral codes, and altruism are facts. Rather than beginning by taking preferences as primitives and allowing all possible preference profiles, formal theory must begin with these facts and accept, as given, certain configurations of widely held preferences that are generated by them.

An ideology is a collection of ideas that exist independently of whether or not anyone accepts them. It tells us what is good, who gets what, and who rules: "An ideology is a mechanism for transmitting information and persuading, a generally consistent set of ideas about what is the 'good' in politics and social intercourse," (p. 19). As such, an ideology implies a world view that can be used to forecast positions on new issues. Consequently, an ideology reduces the transaction costs of political exchange because it acts like a cue, reducing uncertainty and information costs. To know that a politician is a conservative Republican allows a voter to predict that the politician will probably vote against increasing the minimum wage in Congress and vote for "zeroing out" funding for the Corporation for Public Broadcasting. (This predictability is what Philip Converse labeled as "constraint.")

Hinich and Munger represent voter choice directly as ideology: "Citizens judge candidates by the agreement or disagreement between their own view of the 'good society' and that embodied in the candidates' attempts at making clear their ideological positions' (p. 102). Ideology is *not nonrational* nor is it a *substitute* for rationality. Rather, citizens rationally choose between candidates by *utilizing* an ideological comparison in the context of uncertainty and costly information.

The next step in Hinich and Munger's argument is to place their theory of ideology within the framework of the spatial model of voting. They assume that the feasible ideological positions of candidates and parties can be portrayed as points on a small number of ideological dimensions. The number of dimensions will be small both because of the limited physical capacity of people to process information but also because of the nature of ideological competition itself. An orthodox ideology inevitably creates an opposing, or heterodox, ideology. The "heterodox ideology must oppose the orthodox by suggesting intellectual or emotional reasons why the status quo is wrong or unsupportable" (p. 16). Echoing a point made most chillingly by George Orwell in *1984*, Hinich and Munger observe that "the contest is more than one of persuasion: ultimately, the contest is decided by who gets to use their words, their conception, to describe the conflict" (p. 17). (To any who doubt the truth of this statement, the examples are legion: *undocumented immigrant* vs. *illegal alien*," *out-of-wedlock* vs. *illegitimate*, etc.) This battle between opposing sets of ideas and the framing of those ideas, is not a process that leads to complexity. Rather, the heat of the debate tends to create a relevant policy space of only a few dimensions.

In the spatial model of ideology, candidates have fixed positions on the ideological dimensions and the issue positions of the candidates as perceived by an individual voter are assumed to be simple linear mappings from the ideological dimensions onto the issue dimensions. Uncertainty is incorporated into the model by allowing error into voters' perceptions of the locations of candidates in the ideological space. This is the basic spatial model developed by Hinich and Pollard and elaborated by Enelow and Hinich.

The major weakness (or strength, depending on your point of view) of the model lies in the fact that linear mappings from the ideological dimensions into the issue dimensions must be fairly constant across voters. If everyone has their own understanding of ideology, then there is no ideology. This implies a strong empirical test of the model—are the estimated individual mappings the same? Hinich and Munger show some evidence supporting their model (a student survey done at the University of Texas) and discuss other tests performed by Enelow and Hinich that lend further support to it.

Although I personally believe their basic model to be correct, I think Hinich and Munger could make a considerably stronger case for their model by building into it a theory of voter information. From *The American Voter* .o the present, innumerable cross-sectional studies have shown that the level of objective political information (e.g., Which party controls the House?), as well as ideological information (e.g., Is Carter more liberal than Reagan?), varies dramatically across voters. The clear implication is that the individual mappings from the ideological dimensions to the issues are themselves functions of the individual's level of political information. In this regard, the directional theory of voting developed recently by George Rabinowitz (which appears to apply to less-informed voters and not to well-informed voters) may not be incompatible with the ideological model.

The spatial model of ideology is constructed within the rational choice framework. The theory of rational choice is based upon two fundamental assumptions: methodological individualism and purposeful action. Individual purposeful action can be altruistic or self-interested. Indeed, "many irrational actions have important survival values. Organizations able to create norms of behavior (which we have called ideologies) that encourage such 'irrationality' have a powerful advantage in competing with purely egoistic groups" (p. 225). Thus, a purely rational choice argument leads Hinich and Munger to draw some very strong normative conclusions. The whole point of this book is to argue that ideology matters and should be taken as given. Ideology is necessary for political and social life, and political communication cannot exist without it. Consequently, the recent trend toward increasingly negative advertising undermines "the very community of understanding on which any political system depends" (p. 214).

At the end of their book Hinich and Munger state that their primary aim was to convince the reader that existing theory "is simply incapable of expressing, much less understanding, the set of questions that can be asked and answered by the complex concept we have defined as 'ideology'" (p. 237). In this task, I believe they have succeeded.

Carnegie-Mellon University KEITH T. POOLE

Mental Leaps: Analogy in Creative Thought. By Keith J. Holyoak and Paul Thagard. Cambridge: MIT Press. 320p. $24.95.

Rational choice, drawing on a strong microeconomic tradition, has become the dominant model of mind in political science. Meanwhile, the cognitive revolution in a cluster of disciplines has moved in a quite different direction. *Mental Leaps* is an accessible example of such work.

Both authors are professors of psychology, and one has a joint position in computer science. As the subtitle suggests, they have written a book that seeks to develop the importance of analogical thinking for human creativity. They develop a model of *analogic* (p. 5). The model includes a source analog (the model in the thinker's mind) and a target analog (what is to be understood). Analogical thinking involves "establishing a *mapping*, or systematic set of correspondences, between the elements of the source and the target analog" (p. 4).

The analogical process is guided and limited by three constraints: *similarity*, *structure*, and *purpose* (p. 6). Similarity provides the initial plausibility of the match, "the various common properties [that] justify drawing the analogy in the first place" (p. 5). Structure implies "consistent parallels between . . . the source and the target domain" (Ibid.). It includes comprehensive, comparative attribute mapping of the subelements of the source and the target to establish consistent network isomorphism. First-order structural mapping involves "attribute mapping," which is based on attributes of objects" (p. 237). There is a "*one-to-one* constraint: each element of the target domain should correspond to just one element in the source domain (and vice versa)" (pp. 5–6). Second-order "relational mapping" considers higher-order relations between objects" (pp. 77, 237). Third-order "system mapping" rests on higher-order "relations between relations (p. 237). Finally, "the exploration of the analogy is guided by the person's goals in using it, which provide the *purpose* for considering the analogy at all" (p. 6). In the end, coherent "similarity at a more general level emerges as the result of applying the constraints of structure, and purpose" (p. 137). The three constraints together exert pressure "at all stages of analogical development: selection, mapping, evaluation, and learning" (ibid.).

The book is oriented toward the concerns of artificial intelligence. Though it makes little reference to it (and then only superficially and very late [p. 240]), it draws from the earlier tradition of gestalt psychology associated with the work of Köhler, Koffka, and Wertheimer. The developmental psychology of Jean Piaget is included. The problem-solving concerns of mathematician George Polya and the metaphorical theory of linguist George Lakoff and his collaborators also figure strongly. There are multiple examples from different substantive areas—ethology, child development, philosophy, law, the natural and social sciences, and poetry—to develop the book's argument.

The applications of most interest to political scientists center around foreign policy examples, inspired mainly by the work of May, Neustadt, and Khong. There is extensive consideration of the analogy between World War II and the Persian Gulf War, with systematic analysis of various sets of alternate possible correspondences. The authors find that "the analogy generated a 'bistable' mapping: people tended to provide mappings based on either of two coherent but mutually incompatible sets of hypotheses" (p. 105). The outcome depended on how they answered a number of questions? Was George Bush equivalent to FDR or Churchill? Was the Gulf War United States the World War-II-United States or was it World War-II-Britain? Was Saudi Arabia World War-II-Britain or France? Was Kuwait World War-II Poland or Austria? The authors extend to their analysis to consider the use of "World War II as a source analog" in Korea and Vietnam and some laboratory experiments, including World War I (p. 106). They also briefly describe a connectionist computational model of the network structure for the Gulf War (pp. 247–56).

Modeling the mind is now a high-growth industry. There are serious rivals to both the rational choice and the analogical models. Two recent arrivals are based on physical and biochemical traditions. The first of these is Roger Penrose's *Shadows of the Mind* (1994). The second is J. Allan Hobson's *Chemistry of Conscious States* (1994). These books are both competitors and supplements to other perspectives. Space does not permit full reviews, but the books provide particularly interesting perspectives on micro and macro changes in mental states.

Developments in other disciplines inexorably intrude on political science. Our received models of political decision making increasingly contend with alternatives fabricated elsewhere. The evolution of political decision theory and research—and the study of political decisions in different substantive areas and issues—must increasingly incorporate the rich knowledge available in multiple other disciplines. Such an adaptation is necessary if we are to understand political thinking and learning in a truly modern way and if we are to make substantial progress in developing our own contemporary, sophisticated models of the political mind.

University of Colorado, Boulder FRANCIS A. BEER

After MacIntyre: Critical Perspectives on the Work of Alasdair MacIntyre. Edited by John Horton and Susan Mendus. Notre Dame: University of Notre Dame Press, 1994. $39.95.

Since 1981, Alasdair MacIntyre has published three books on moral and political philosophy that have been distinguished by two overarching themes. The first has been a studied hostility to modern moral discourse, which, in MacIntyre's view is characterized by interminable debates and irreconcilable disagreements emerging from its basic incoherence. Modern moral philosophy and (to a large degree) practice consist of fragments of past moral traditions that have been ripped from contexts where they made sense. Partisans in modern moral debates take up sides around incommensurable premises and can neither persuade each other nor justify their own moral stances as anything other than emotive preferences or acts of will.

This evaluation of the sorry state of modern ethics leads MacIntyre to consider how and under what conditions moral thought can make sense. Thus, his second main theme is an epistemological one developing a position that is historicist though not relativist. MacIntyre's first book in this period, *After Virtue*, was dedicated largely to the first theme, while the other two—*Whose Justice, Which Rationality?* and *Three Rival Versions of Moral Inquiry*—deal largely with the latter. In all three, however, MacIntyre develops the view that rational justification in morals is tradition-dependent, a tradition being an ordered collection of social practices, analogous to crafts, each with its own standard of excellence. Traditions link contemporaneous practices enabling participants to view their lives as narrative wholes—as stories that, unlike modernist fictions, have beginnings, middles, and ends all in the right order. They also allow for the passing on of these standards across generations. Moreover, MacIntyre claims that the "Enlightenment Project," which defines contemporary moral thought, was destined to fail because it sought extratraditional moral justifications. Hence, in *After Virtue*, MacIntyre offers a stark choice between the tradition-bound reasonings of Aristotle or Nietzschean nihilism.

The critical essays in this collection (which includes a response by MacIntyre and a bibliography of his writings) fit into three nonexclusive categories. The first evaluates MacIntyre's interpretations of emblematic thinkers and/or traditions of discourse from Aristotle through Aquinas and the Scottish Enlightenment. The second type deals largely with whether the author's epistemic views can be sustained without lapsing into relativism or dogmatism. The third set evaluates MacIntyre on modern liberal, utilitarian, and feminist theories, generally arguing that these schools of thought have greater moral resources for generating reasoned consensus than their subject allows. The essays are of high quality and are more often critical of, than sympathetic with, the author, though he has his defenders. One small problem is that the recounting of similarly understood basic terminology in almost every essay grows tedious (though an essay on MacIntyre and feminist thought does seem rather confused about his notions of a practice and tradition).

Among the most thought-provoking essays is Charles Taylor's, arguably the most interesting contemporary Aristotelian. Though Taylor largely endorses Mac-

Intyre's view of moral reasoning, he is more sanguine about modern morality. This is not because he likes the philosophies of it much better but because he is convinced that they inadequately describe the practice. For Taylor, moral reasoning is necessarily Aristotelian (e.g., grounded in notions of human good), and there is only so much philosophers can do to mess this up. At the same time, Taylor is also drawn to modern notions of contingency, choice, and critical distance. The ironic thought he provokes is that modern Aristotelians (or Thomists) arrive at that position by viewing their moral world from a critical distance they deride, while that world itself may be more Aristotelian than the crankier among them (including MacIntyre) wish to admit.

Among the exegetical works, Janet Coleman's piece, "MacIntyre and Aquinas," is the most important. Coleman carefully takes us through Aquinas' epistemology summing it up in one stark sentence: "Reason overrides custom." (p. 87) She argues convincingly that this view is a good way from MacIntyre's case for tradition-bound reasoning. In response, MacIntyre minimizes his differences with Coleman's reading of Aquinas but maintains, in essence, that some traditions of discourse are better situated to see things as they really are than others and that this is Aquinas's strength. To be generous, this is a difficult case to make; and MacIntyre does not make it in his brief response but promises to in a future book.

To be sure, MacIntyre's next book also will be read with great interest; but judging from the track record of these essays, it will have limited power to persuade his current critics. Yet given MacIntyre's view of modern moral and political thought, this is not only what he would expect but probably even what he would want.

University of California, Davis RICHARD C. SINOPOLI

A Political Theory of Rights. By Attracta Ingram. Oxford: Clarendon, 1994. 232p. $59.00 cloth, $29.95 paper.

By a "political" theory of rights, Attracta Ingram means a conception of rights as based on self-government or autonomy insofar as it is associated with citizenship in a liberal democracy. This is contrasted with a "proprietary" conception of rights as based on self-ownership with its accompanying economic practices and institutions. The first part of her book presents acute arguments against the self-ownership conception, while the second, larger part works out a detailed development of the political conception.

Let it be said at once that this is a very good book. Although I have some difficulties with various of its phases, overall it provides a set of cogent, thought-provoking analyses and arguments about fundamental issues of the nature and contents of rights.

Ingram states the central point of her project as follows: "The orthodox conception of rights—the libertarian view that rights are a species of moral property in one's person, personal powers, and legitimately acquired external resources—belongs in a theory of justice that fails what is now a widely accepted criterion: justice is what everyone could in principle reach a rational agreement on" (p. 3). So she first takes up the plausible, widely accepted idea that each person has a certain inviolability as being the owner of her person and powers. From this, libertarians infer that to have a right is to have property in or "proprietary control" over oneself and what one does with oneself, so long as one does not harm other persons.

Ingram subjects this idea to a battery of devastating criticisms. For one thing, such a conception of rights, paradoxically, is compatible with slavery; for by making humans the objects of property rules, it sanctions voluntary enslavement. Moreover, the conception authorizes drastic economic inequalities, for the unfettered use of private property rights in one's own person and powers allows the more able persons to accumulate a major share of worldly resources. On similar grounds, the self-ownership conception rules out welfare rights, for it denies that some persons' resources can justly be taxed to provide subsistence for other persons. So the conception can lead to severe dependency on the part of persons who need help but do not receive it, so that they are not self-owners.

In contrast to self-ownership, Ingram upholds a self-government conception of rights as based on personal autonomy. At first glance, it may seem that there is little or no difference between the two conceptions, for does not self-ownership entail self-government? On the contrary, Ingram, with subtle but forceful analysis and argument, brings out major differences. They stem from the consideration that autonomy consists in making laws or rules for oneself; and because laws have an implicit generality, they apply to the conduct of many or all other persons. Thus, in contrast to the confined privacy and exclusiveness of the proprietary conception of rights, the political conception based on self-government requires that rights be the results of the universal consent or agreement that is a hallmark of justice. Ingram devotes several chapters to a careful delineation of the "process of social dialogue" that should yield such agreement. The result is a conception of rights that makes ample provision both for the civil liberties of political democracy and for the economic, social, and cultural rights of social democracy.

I am in substantial agreement with Ingram's normative conclusions and with many of the arguments she adduces in their support. But I have some worries on particular points. For one thing, she is too ready to concede the transition, upheld by Locke and contemporary libertarians, from self-ownership to each self's exclusive ownership of external goods and wealth it has produced. Surely, one does not "own" one's money in the same way one "owns" one's body or mind. It is belaboring the obvious to point out that one's self, including one's powers, is intrinsically united to oneself in a way that does not characterize one's external products or possessions. From confusing these two kinds of ownership (epitomized in Locke's frequent use of *property* to refer to life and liberty, as well as to "estates") stems the libertarians' contention that taking my money through taxation is on a par with removing my arm or my leg. Calling attention to the incoherence of this inference would have enabled Ingram to retain the valuable aspects of self-ownership as involving personal inviolability without yielding to its presumed antiwelfarist implications. The self-ownership of the affluent is not violated if they are taxed to relieve the severe needs of the poor while retaining a surplus that leaves them more than fully capable of meeting their own needs.

Ingram upholds a "constructivist" conception of rights whereby they are not objective features of the world but, rather, results of a process of agreement motivated by liberal democratic concerns. While this view gives her approach an appealing proceduralist and circumstantial basis, it also incurs difficulties. For one thing, it raises the problem of implicit circularity; for she seems both to justify liberal democracy through its upholding of political and social rights and to justify the rights through their being central values of liberal democracy. In addition, her circumstantialist approach raises problems for the universality of human rights, which she emphatically upholds. Her valiant attempts to reconcile the apparent conflict between particularism and universalism in this sphere leave much to be desired.

Ingram analyzes the process of "constructing rights" by first subjecting to careful scrutiny the doctrines of Rawls, Habermas, and others and then "building in" a series of "conversational constraints" on the "ideal discourse" which should lead to the establishment of rights as based on autonomy. The constraints are both personal (including equal respect, reasonableness, generality) and political (including symmetry, "secularity," and universality). Much of this is very well done. But despite its moral specificity, it also emits an air of idealistic unrealism. It would have been helpful if Ingram had confronted her moral norms with the problems raised by the realities of actual democratic practices, with their inequalities of political power. Can the distinction between self-ownership and self-government adequately account for the contrasts she rightly decries between libertarian economic inequalities and the ideal of the equal moral worth of persons?

These and other criticisms one could make of Ingram's performance do not appreciably detract from the great value of her book. It is an excellent contribution to a vitally important subject, and it requires and deserves careful study.

University of Chicago ALAN GEWIRTH

The Limits of Hobbesian Contractarianism. By Jody S. Kraus. Cambridge: Cambridge University Press, 1993. 334p. $49.95.

The Limits of Hobbesian Contractarianism is essentially three extended essays about Jean Hampton's *Hobbes and the Social Contract Tradition* (1986), Gregory Kavka's *Hobbesian Moral and Political Theory* (1986), and David Gauthier's *Morals by Agreement* (1986). These three books have been subjected to extensive reviews, and Gauthier's has been treated to more reviews and commentary essays than even Gauthier might wish to read. A discussion of Kraus's book threatens to be a review of reviews, which would suffer from the law of diminishing fleas (microscopic fleas feeding on Kraus's fleas feeding on the original curs, two of which (the books of Hampton and Kavka) were themselves primarily commentaries on Hobbes, one of the grandest of curs. Kraus partly elevates his work with a general introduction on how contractarian political theory is and should be done, but even this is done with principal reference to the three authors under his review.

Since all of these authors exhibit great intelligence and often elegant argument, they all repay reading. A systematic treatment of Kraus's book in a few paragraphs is, however, not possible. Explicit in Kraus's account is that his three authors have failed to make such theory compelling; implicit is that such theory may be hopeless. He concludes that Hobbesian contractarians have failed to give a normative motivation for the kind of hypothetical justification on which their theories depend. This is the overriding focus of his account. He states that all political contractarian theories must explain the *normative* significance of hypothetical results (p. 184). This is true only if we suppose that the point of hypothetical contracting is normatively to justify current obedience to a regime. This is presumably analogous to the cavalier conclusion from an actual agreement on establishing a government that—if we contracted—we are now morally obligated to obey. Hobbes's contractarian account includes none of this assumption of a normative obligation of the kind that many recent moral theorists insist follows from *promising*.

The problem that drove Hobbes's account was the revolutionary struggles of his own time. These were not resolved by a social contract, nor did Hobbes argue that they should be. Rather, he argued that we should be obeisant even to a state that has come about by conquest or coup. One might object that ordering a population by conquest is just politics, while contractarianism is philosophy. But politics was Hobbes's central interest. He wished to justify obeisance to the state, to argue against rebelling merely for the sake of improving one's lot. His justification was simply that it could seldom, if ever, be in one's interest to attempt revolution. He needed no recourse to moral argument or to a moralized notion of obligation. Interest and obeisance were sufficient.

Hobbes's fable of the social contract was a sociological—not a moral—move, and it was evidently a sociological failure even in his own view. He saw the crucial problem of enforcing the original contract once agreement was concluded (in *De cive*), then dodged it (a decade later in *Leviathan*). Hampton and Kavka clearly understand that this problem of empowerment is severe and that even if we contract, we still face the grim logic of collective action when our new sovereign attempts to bring us to order. *Rational* contractarianism falters on this point and no one has proposed a serious solution to it. Hampton seemingly resolves it in her argument that Hobbes mistakenly views interactions in the state of nature as essentially one-shot prisoner's dilemmas, whereas they must often be iterated. Since it is rational to cooperate in iterated prisoner's dilemmas, the state of nature is less grim than Hobbes saw it. Against her argument to get rational contractianism off the ground, however, it is typically rational to cooperate in iterated prisoner's dilemma only for two or very few players. Hobbes seems to have recognized the rationality of cooperating in ongoing relationships, but he supposed that the chief problem was interactions with the many when few of these could be ongoing. Two of us would not need to contract in order to cooperate; the many cannot cooperate without external coercion. Hence, the need for the state.

Strangely, of the authors under Kraus's review, the one who most identifies with Hobbes's theorizing—from interest rather than from morality—is Gauthier in his Hobbesian account of personal moral theory. To focus on dyadic interactions, he resorts to the dubious trickery of supposing (1) that we can rationally dispose ourselves to be cooperative with those who will likewise be cooperative and (2) that we can somehow identify those with the relevant disposition. (In iterated interactions, these assumptions would be plausible merely from self-interest, but Gauthier resolutely focuses his theory on one-shot interactions.) Few critics have accepted either of these moves. Kraus argues that these moves do not yield Gauthier's conclusion that we would rationally comply only with fair bargains. Hence, they do not yield a compelling moral theory.

New York University RUSSELL HARDIN

If Men Were Angels: James Madison and the Heartless Empire of Reason. By Richard K. Matthews. Lawrence: University Press of Kansas. 1995. 320p. $25.00.

This is an interesting and well-written book worthy of careful consideration by students of political theory and American history. The author sets as his objectives: (1) "to construct the complete political theory of James Madison," (2) to provide "a critical analysis of Madisonian politics," and (3) to understand "Madison's politics" in light of "what America has failed to become" (pp. xvii–xviii). Eight chapters beyond the introduction are concerned with Madison as a person, his world view, his conceptions of humanity, civil society, property rights, government, Jefferson on Madison, and brief concluding reflections built around Robert Frost's metaphor of reflection as "knitting." As a student primarily concerned with Madison's theory of a "compound republic," I especially value Matthew's examination of the extended body of Madison's work in a sequence of topics of fundamental importance to the constitution of order in human societies. The chapters on civil society, property rights, and Madison's system of governance are especially helpful.

The best of works deserve critical reflection in the interest of continuing dialogue and inquiry. I am not convinced that Matthews has gained a sufficiently sympathetic understanding of Madisonian thought to appreciate how the system that Madison helped to conceptualize might be made to work. No formulas, conceptions, words on paper, tools, or gifts work by themselves. Knowledge, skill, and intelligence are necessary to make any human creation work. Conceptualizing how a system might be made to work is a necessary prerequisite both for specifying a theory and rendering a critical analysis.

Among the commentators on the American political system who have achieved a high level of sympathetic understanding about how such a system could be made to work and the potential sources of its failures, I would list Alexis de Tocqueville first. Francis Lieber and Moisei Ostrogorski are worthy of serious attention. Tocqueville, in referring to the distinctive institutional arrangements contributing most to the viability of the American system, gave first place to the federal system at the core of Madison's efforts, to the institutions of local governance, and to the judiciary. A primary preoccupation with the federal constitution is clearly insufficient for conceptualizing essential features of the American political system.

Matthews uses the concept *state* as though a political theory were a theory of the state. Tocqueville, by contrast, viewed the American system of governance as being grounded in the conception of a self-governing society rather than a state-governed society. The art and science of association were, for Tocqueville, the foundations for citizenship and for the politics of a democratic society and include reference to a general theory of limited constitutions that was of critical concern to Madison. I accept Matthews's commentary that Madison neglected several essential topics, including citizenship and suffrage, in his life's work. I also recognize the great anomaly of a person deeply committed to freedom living in a society that practiced slavery. We do not choose the time and place of our birth and many of the circumstances in which we live our lives.

These issues are important because Matthews repeatedly refers to the demos as provoking anxieties or fears for Madison without indicating what it might mean for the *demos* to exercise *kratia* (power), as implied by the term *democracy*. Perhaps Madison was correct in his reluctance to assert a "right to revolution" and to "call for revolution, revolt, or even legitimate violence" (pp. 153, 175). The French Jacobins had no such reluctance; and the First and Second Republics were very short lived amid frequent changes in types of political regimes. The average tenure of governments in the Third and Fourth Republics was less than one year. Recourse to "legitimate violence" by "populists" in nineteenth-century Russia and communists in twentieth-century Russia has not served the *demos* very well.

The problem of conceptualizing the nature of property rights poses such a difficult problem that neither is Matthews nor am I likely to achieve resolution. Madison, according to Matthews, insisted that concepts of property and authority cannot be disassociated. Matthews relies on the concept of *market* to attempt to indicate a narrower concept of property. Unfortunately, what might be conceptualized as private property does not include other forms of property which do not necessarily coincide with the boundaries of nation-states (i.e., common-pool resources and public goods involving potential forms of common wealth, or commons). How wealth is to be redistributed without monetized media of exchange is difficult to imagine. And monetary transfers can only give access—without sympathy, compassion, or helping hands—to the type of goods available in a market economy. Further, any society needs to distinguish between consumption goods and *capital* (goods or property used to produce other goods). Every society has a great stake in assigning authority for the proper use and care of its productive capabilities, including the skills of its population. The "poor," as such, are not the best repository of productive assets. The complex configurations of property right relationships are not satisfactorily addressed in market theory nor by Marxist allusions to the good of each being related to the good of all, as though the world were a simple porridge equally accessible to all.

The genius of Madison's formulations was to use and extend Montesquieu's concept of using power to check power as a means of constituting order in human societies. Any such arrangement is potentially subject to stalemate, and any ex ante effort to avoid potential stalemates requires some form of autocracy. The key question is whether human beings can take time out to pursue problem-solving strategies in the presence of conflicts as means of achieving conflict resolution, rather than allowing conflict to escalate to violent confrontations. The demos has no godlike capabilities, and is required, like everyone else, to use knowledge, skill, and intelligence to cope with problems. The organon for achieving problem-solving capabilities is the mind. Reason is an essential complement sentiments like sympathy in Madison's "heartless empire of reason." What is considered to be anxiety, fear, or prudence may be difficult to determine. *If Men Were Angels* is worthy of both sympathetic study and critical reflections.

Indiana University VINCENT OSTROM

Socialism for a Sceptical Age. By Ralph Miliband. New York: Verso, 1994. 211p. $64.95 cloth, $18.95 paper.

Completed shortly before the author's death, this book is his passionate entreaty to continue the struggle for socialism. Miliband's socialism has two intertwined objectives: "democratization far beyond anything which capitalist democracy can afford; and egalitarianism, that is to say the radical attenuation of the immense inequalities of every kind which are part of capitalist democracy" (p. 3). Furthermore, the implementation of these two objectives will require the socialization of a predominant part of the means of economic activity, which the author elaborates to consist in (1) nationalization of the "commanding heights" of the economy and (2) collective ownership by various associations of much (but not all) of the remainder.

The book may be divided into three parts. The first, a discussion of the ills of capitalism (including a highlighted history of signal events of the twentieth century), argues that capitalism has at once created the material possibility for a magnificent flowering of human potential and failed to fulfill that promise. The second tells in detail what socialist democracy is and how a socialist economy would be organized to engender equality of condition (or at least vastly more such equality than capitalism delivers). The last part concerns the survival of a socialist regime (what constituencies would support it and what kind of national and international political strategies the regime would undertake), taking the experience of European social democracies as a kind of natural experiment. In my view, the first part is on target, the second falls far short of being social science, and the third is too hypothetical to be of much interest, especially given the lack of convincing elaboration of the socialist mechanism in the second part.

The first part is a useful recapitulation of the class nature of capitalist society. The concept of democracy, Miliband argues, has been emasculated and appropriated, in its amended form, by capitalism. While democracy should be taken to mean that "ordinary people have real power in all the areas of life where decisions that concern them are made," it has come to mean the "competition between political elites for power to be achieved on the basis of popular support as expressed in competitive elections" (p. 25). That "ordinary people" are in large part unrepresented in bourgeois democracy is due, in no small part, to the ownership and control of almost all the media by large capital. It is remarkable, Miliband says, that the almost complete ownership of the most important media by large capitalist interests should be taken to be compatible with democracy. (Under socialism, there would be publicly owned media; media owned by collectivities [unions, associations]; and privately owned media, with the restriction that no individual own more than one newspaper or radio station.)

Miliband takes a traditional Marxist view on American imperialism that the guiding principle of postwar U.S. foreign policy has been to defend capitalist regimes from not only revolutionary challenge but even radical reform (Greece, Iran, Guatemala, Brazil, the Dominican Republic, Indonesia, Vietnam, Chile, Nicaragua, and Cuba). The aim of making the world safe for capitalism entailed a policy of supporting capitalist regimes of a particularly noxious kind. Despite being a repressive dictatorship, the USSR was not an expansionist regime. Notably, it was not involved militarily in the two hot wars, Korea and Vietnam.

Like almost all Western socialists today, Miliband views Soviet socialism as a perverse distortion of the real thing. I need hardly elaborate his view of real socialism, consisting of thoroughgoing democracy and egalitarianism; readers are familiar with it from any number of socialist writings. The tenet underlying the possibility of such a society is a "fundamental optimism about human capabilities, . . . the belief that human beings are perfectly capable of organizing themselves into cooperative, democratic, egalitarian, self-governing communities" (p. 58). Miliband holds this tenet, arguing that the revealed capacity of people for greed, hatred, and mass killing and oppression is still consistent with human malleability and, indeed, perfectibility.

Even granted that human nature is not the ugly thing that conservative political philosophy portrays, a scientific approach to socialism must consist in an elaboration of the mechanisms that a society could design to engender thoroughgoing democracy and substantial equality of condition. This approach is absent from Miliband's analysis. With respect to thoroughgoing democracy, there are two immense problems: (1) how to resolve the ubiquitous conflicts that would emerge when citizens with different and legitimate interests disagree about some policy and (2) to discover the optimal trade-off between representation and participation. Only a utopian could believe that the first problem will vanish with the abolition of private ownership of the principle means of production. Scarcity of resources (including nature) is a reality, not a construct of capitalism, and will induce competition for those resources even without highly concentrated ownership of firms. I share Miliband's hope that absent capitalist property relations, there is a possibility for rational sharing of resources; but much more than an invocation of the human capacity for cooperation is needed. On the second point, the problem is how to organize the political decision-making apparatus so that "true representation" occurs. Principal-agent language is useful here. Between the ultimate collective principals (the citizenry) and the executive branch of the government (their agent) there must be a number of layers of representation. Roughly speaking, the more layers there are, the more consuming in resources (including, as Oscar Wilde said, free evenings) is the decision-making process. The fewer the layers, the more difficult is the monitoring problem: How can each layer of principals ensure that its representative does his or her job honestly and vigorously. Regretfully, I cannot say that Miliband has anything new to contribute on these two points. To believe that their solution will be more or less natural once capitalist interests have been dissolved, I believe, is to trivialize the complexity of governance in a large, heterogeneous society.

Miliband's recipe for the economy is traditional: socialization would entail the public ownership of large enterprises. He defends (largely correctly, in my view) the claims that (1) publicly owned firms can be at least as efficient privately owned ones and (2) government intervention in the economy is necessary for growth, as well as equity. A general conception of how the economy would work, however, is lacking. Evidently, public firms would not be encouraged to maximize profits, for Miliband repeatedly attacks profit maximization as responsible for many of the ills of capitalism. How, then, do public firms decide what to produce and how to produce it? If the government is always to have the final say with regard to public production, as Miliband suggests, how will the "soft-budget constraint" phenomenon be avoided? Hayek perceptively noted 60 years ago that government intervention in the operation of the firm renders it impossible (or at least, much more difficult) for it to hold firm management accountable. Many contemporary market-socialists argue that the key to an efficient economy is a credible commitment by the state *not* to interfere in the decisions of firms and that such credibility can only be created by designing an ownership structure of firms in which the government is only one of many institutional owners. Although Miliband states that public-sector enterprises would enjoy a high degree of autonomy in running their affairs (p. 110), the lack of any specific property relations to guarantee this and his simultaneous emphasis on the desirability of the government's having the final say on all economic matters belie the claim. Granted, the French public sector has been admirably run. But we cannot depend on most (future socialist) countries' having had the long public corporate culture that France has had, not to speak of the frequent migration of top management between the private and public sectors characteristic of France.

Ralph Miliband's vision is a fine one, and these days it pays well to be reminded of it. It is useful, as well, to expose the unsubstantiated claims of right-wing propaganda—that individual greed is the only basis on which society can be successfully designed and that everyone can become rich gloriously under capitalism but for his own weakness of spirit and will. But an argument for the possibility of socialism requires considerably more hard analysis than Miliband has given us.

University of California, Davis JOHN E. ROEMER

Ethics in the Public Domain: Essays in the Morality of Law and Politics. By Joseph Raz. Oxford: Clarendon, 1994. 364p. $60.00.

This collection of essays is divided into two parts. The first of these contains essays in which Raz attempts to defend the approach to political morality he adopted in *The Morality of Freedom* (1986)—that "political morality is concerned primarily with protecting and promoting the well-being of people"—against "opposing arguments which restrict politics by imposing constraints of so-called 'neutrality among competing conceptions of the good,' or which regard rights as constraints on political action whose force derives from considerations which are unrelated to individual well-being" (p. v). The latter part includes essays that sketch Raz's view of the nature of law and consider the consequences following on this view. Here, Raz argues that "the content of the law is capable of being identified by reference to social facts only" and that law not only regulates conduct and authorizes the creation of new laws but also "sets objectives that future legislation should achieve, objectives which regulate not the procedure of law-making but its content" (pp. vi–vii).

Raz's strengths are closely wedded to his weaknesses. A central strength of his theory is its acutely logical structure and elegance. But this is bought at the price of a high level of abstraction and detachment from reference to practical politics. Indeed, although one cannot help but admire Raz's facility at constructing beautifully crafted arguments, one also cannot help but feel dismayed that so much effort has so often gone into the production of airless theoretical castles, indifferent to embodied living. In my case, this regret was quickly displaced by frustration with Raz's frequent failure to discuss in concrete terms the practical implications of the theories he advanced.

The effect of this detachment is to often leave the reader with the sense that Raz's discourse is a conversation with himself, proceeding in a context of self-absorption in puzzles of his own imagining. One might even find oneself wondering if one is reading political argument or, instead, eavesdropping on a recitation of the articles of some wholly theoretical liberal faith. This impression is, however, mistaken. Arguments that at first seem to be only tautologies affirming that liberalism has indeed constructed the best of all possible social worlds, turn out to be not an endorsement of the prevalent liberal orthodoxy but instead an attempt to allow the liberal state from the demand that it be neutral as to conceptions of the good. Raz's is a perfectionist liberalism, a liberalism emboldened by an almost-quaint confidence in its own rationality and willing, on the basis of this confidence, to endorse expansive state action in the advancement of its vision of the good.

An example of this is found in the essay "Rights and Individual Well-Being," in which Raz seeks to establish that the "generalized view" that rights "represent the individual's perspective or interest against the general or public good" is wrong (p. 29). Instead, Raz avers that when people "make substantial sacrifices in the name of one of the fundamental . . . rights of an individual, this is not because in some matters the interest of the individual" prevails over the interest of the majority but "because by protecting the right of that individual one protects the common good and is thus serving the interest of the majority" (p. 38). But isn't this the stuff of junior high civics classes and Kawanis luncheons? Surely, liberal ideology holds that we are all better off if the rights of each of us are protected, but can Raz be intending only such bland reaffirmation? Not at all. Instead, his object is to show "that the common good served by . . . rights is, in the majority of cases, more important to individuals than the enjoyment of their own civil and political rights, and therefore that the status the rights enjoy in liberal democracies is due to their contribution to the common good" (p. 38).

It is at moments like this that Raz's failure to spell out the implications of his positions is particularly acutely felt. He goes on to give some examples apparently aimed at clarifying his central point, but they do little to allay our anxieties over the apparent direction of his remarks. One of these is the case of "freedom of contract," which he says tends to "an environment [whose existence] is a common good" (p. 38). Even young children, who do not make contracts, "benefit from the fact that they live in a free society in which people generally have power to control their own affairs;" and this is "a typical case of individual rights playing a major part in securing the existence of the common good" (p. 39). Perhaps so, but what about adults whose freedom of contract conflicts with some prevalent view of the common good? Raz writes that because "there is every reason to regard the value of the rights to the common good as part of their justification," such consideration of the common good is "also a factor in determining the weight of rights" (p. 40). How much of a factor? He fails to say.

Nor does he offer reassurance by his discussion of what constitutes the common good. In this case, although he acknowledges that the "politics of the common good . . . are as controversial as other political issues," he insists that "they are relatively free from conflict" because we all "share a common interest but [only] disagree on the best way to pursue it" (pp. 41–42). But he is able to say this only because of the extreme abstraction of his account. We all do have an interest in living in a free society; but having said this, what have we really said? Little that can help us decide whether our common interest in freedom is served by letting everyone contract as equals or by mandating protections for certain classes of contractors. Is a woman more free if she can contract out her services as a surrogate mother or if she is barred from such contracts but allowed to languish in poverty with her dignity intact? How do we measure well-being in such circumstances? Indeed, could even the woman so affected tell us in what direction her well-being lies?

Raz writes that issues of the common good "are to be settled on the basis of the central tenets of the political tradition of the country concerned" (p. 42). But which of these traditions is not infected with masculinism, racism, classism, and heterosexism? Sometimes rights are all that we have to protect us *from* traditions so diseased. Only far above the fray could one feel as confident of one's reason and as untroubled in one's authorization of state action as does Raz.

University of Utah EILEEN BRESNAHAN

The Myth of American Individualism: The Protestant Origins of American Political Thought. By Barry Alan Shain. Princeton: Princeton University Press, 1994. 394p. $39.50.

A decade or so before the outbreak of the American Revolution, the good citizens of Edinburgh, members of the Edinburgh Belle Lettres Society, were asked "whether the Character of Cato or that of Atticus is the most excellent." The purpose of this question was to ascertain the true residence of virtue, whether in the public or the private life. For the literati of Edinburgh society, the answer was overwhelmingly to be found in Atticus' role as a virtuous, private individual—an impartial and sympathetic observer of the public realm.

Barry Alan Shain's book is a long, tightly knit and scholarly argument that whatever may have been true in Scotland at that time, the true residence of virtue was manifestly not the private life in the 13 colonies (especially not in New England). Arguing against the liberal-individualist reading of eighteenth-century American political thought identified with the writings of Louis Hartz and Yehushua Arieli (among others), Shain claims that in the second half of the eighteenth century, American political thought was at one and the same time communal, local, and rooted in the traditions of reformed religion. The issue at stake (and the stakes are admittedly high, given current political debate in the United States) is the conception of the public good prevalent in eighteenth-century America. Shain's answer to this is unequivocal (perhaps too much so) and rests on the axiom that the good of the public took precedence over any ideas of individual rights or liberties. The book is structured around a defense of this proposition through an explanation of how three eighteenth-century political traditions (Whig republicanism, reformed Protestantism, and Enlightenment rationalism) all coalesced to privilege public good over private or individual rights.

To be sure, the author's defense of a communalist locus in eighteenth-century American thought is not new and, in some respects, follows on the scholarship of such historians as J. G. A. Pocock and Bernard Bailyn. However, in addition to repeating oft-voiced arguments on the classic republican tradition in America, Shain broadens our understanding of this tradition to alert us to the inherently local and often familial values held by eighteenth-century Americans. Indeed, it is in local loyalties and commitments that the author seeks the full force of that very American communal tradition which, well into the nineteenth century, mediated all attempts to articulate a political vision premised on the moral autonomy of the individual (and its concomitant package of liberties, privileges, and entitlements).

This facet of the author's argument is perhaps the most original and significant, as it stresses the importance of secondary or intermediary organizations standing between the individual and the collective as the basis for collective identity and its vision of the public good. Unfortunately, the very cogency and persuasiveness of this argument raises questions as to the broader counters of the author's thesis; for loyalty to commune and to country are not of a set. A view of political virtue rooted in the parochialism of family ties and local constraints is very different from that republican tradition expressed in Cato's *Letters* or in the idea of representative institutions that we identify with Whig radicalism. It is equally distant from those Renaissance traditions of civic humanism within which the author seeks to root the communal orientations of eighteenth-century Americans.

Now there is a sense that the author is aware of the differences between these political visions. Rather than graft American localism onto Renaissance republicanism, he sees the former as stemming from the more nativist (and often religiously oriented) communities of seventeenth-century "covenanted saints." This reading of the American local tradition, however, begs more questions than it answers; for the reader is never treated to an analysis of how the very different strands of political thought analyzed (and the narrative is rather weak on the rationalist strand) coexisted in the minds of eighteenth-century Americans. Not only do local loyalties and republican traditions stand in tension, the one with the other, but—and perhaps more problematically—the coexistence of reformed Protestantism and Whig republicanism is more a historical problem to be explained than a simple fact to be stated.

The religious corporatism of the seventeenth century, replete with its apocalyptic tensions, was far removed from the deistic and slowly rationalizing world-view of the eighteenth. One cannot simply extrapolate the one from the other. For example, the whole process whereby communal identities were transformed from religious to secular ones and definitions of individual virtue transposed from an idiom of grace to one of (secular) morality is never addressed. Yet without understanding this transformation, any real appreciation of the Protestant origins of American political thought remains severely truncated. Along similar lines, the author fails to disaggregate the American Protestant tradition; and the crucial debate over the Arminian (as opposed to enthusiastic) roots of eighteenth-century political and social thought (and the crucial role of the Great Awakening therein) is never addressed. The "civic-synergism" of a Charles Chauncy and Jonathan Edwards' intimations of the apocalypse were, after all, very different (as were their ideas of original sin). As the author devotes a good deal of space to how the idea of a corrupt human nature influenced eighteenth-century political thought, such distinctions would seem to be crucial.

For the thoughtful reader, however, there is much to be found in these pages, both in the claims made and the questions these claims raise. Given the continuing debate among scholars (and citizens) over visions of the public good and over communitarian or liberal-individualist versions of the social order, it is good to be reminded of both the historical sources of these ideas and their inherent intractability.

University of Colorado, Boulder ADAM B. SELIGMAN

Rethinking Democratic Education: The Politics of Reform. By David M. Steiner. Baltimore: Johns Hopkins University Press, 1994. 253p. $32.95.

Dissatisfied with the dominant proposals for the reform of American education (especially the language in which those proposals are cast), David Steiner has undertaken a "recovery and reforging of a vocabulary for democratic education" (p. xiii) that aims to relate epistemology and political theory to educational practice through a synthesis of Protagoras, Rousseau, and Dewey. Steiner begins his innovative book by cogently arguing the need for a national concern with the substance of civic education, faulting some democratic theorists (e.g., Benjamin Barber and Amy Gutmann) for their preoccupation with procedural questions. He clearly points out inadequacies with the *efficiency school* of reform, which sees no educational goal beyond economic competitiveness. The current emphasis on preparing most students for high-tech jobs probably overestimates the future number of those jobs. More important, the linking of educational policies to prevailing economic conditions and needs produces confusion in schools. A decrease in the dropout rate and an increase in test scores are desirable, and Steiner attempts to give the efficiency school its due for promoting those objects. On the issue of parental choice, however, he unfairly chides those who decry the breakdown of families and yet make the family the central source of educational authority over their children. He neglects the possibility that assigning greater mutual responsibility to the members of the family is indispensable for reversing its decline.

The cornerstone of Steiner's theory of democratic education is Protagoras' doctrine "Man is the measure of all things" (Plato, *Theaetetus* 152a). We leave aside the question whether Protagoras' convictions are truly democratic, which Steiner recognizes as problematic. Steiner allows the doctrine to stand without directly answering Socrates' criticism of it: Protagoras claims that whatever a person believes to be true is true for that person, but Socrates counters that people believe that belief is not a sufficient condition for truth (170ab). Yet in effect, Steiner employs Socrates' criticism when he says that part of the necessary educational work of Protagoras and the other sophists is to persuade citizens to accept as their own the opinions that have traditionally proved most productive of political concord. Man is not simply the measure, after all. The other part of the sophists' task is to instruct concerning rhetorical techniques so that citizens may judge their leaders' competing speeches. But the tension between those two parts will likely cause the sophists' project to fail. If citizens come to believe that language is merely "a measuring rod that is internally transparent, reflecting its own shape" (p. 55), will not respect for tradition evaporate into cynicism?

Steiner's two chapters on Rousseau's contribution to democratic education offer a number of very insightful observations, particularly concerning the relation between the natural and civil themes of his writings. The similarity between the work of the tutor with Emile and the work of the Legislator with the citizens in the *Social Contract* lies in the effort to impose limits and self-denial on the pupil or citizen while deceiving him into believing that he has chosen those limits himself. But as Rousseau teaches the need for externally imposed limits, we move farther away from the standard of "man as measure." We may doubt that it is wise to follow Steiner in making Rousseau's own life a model for others; as he admits, Rousseau was not the perfect democratic citizen. But he also demonstrates that this "thinker in extremity" (p. 123), who shows the impossibility of achieving either natural or civil freedom in the extreme, is helpful for democracies to have around.

Steiner's reading of Dewey succeeds (where recent commentators have often failed) in indicating the foundational role of Dewey's "naturalistic metaphysics" (esp. Dewey's notion of existential "continuity" between humans and their environment) for the rest of his philosophy, including his political thought. For Dewey, democracy as a way of life is incompatible with a metaphysical dualism that dichotomizes mind and world. But Steiner's dependence on Dewey's metaphysics to support his theory is highly questionable. The metaphysics enables Dewey to sidestep the epistemological labors bequeathed by Descartes (for which no one should blame Dewey), but he claims to have arrived at his metaphysical conclusions by the scientific method ("Half-hearted Naturalism," 1927). Dewey's fundamental conviction, then, is his faith that science is *the* way to arrive at truth. Steiner concedes that in light of Thomas Kuhn and others, Dewey's faith can "strike an awkward note" (p. 147). I say that Dewey's faith is unfounded—hence his metaphysics, too. Derived from his metaphysics are his conceptions of "experience" and "growth," also pivotal for Steiner's theory. Those notions, which presuppose that there are no fixed moral standards, often cause chaos when applied at the lower levels of education. Moreover, they are unhelpful at the highest level for considering whether there are fixed moral standards.

In the final chapter, Steiner makes several prudent recommendations concerning educational policy (e.g., that students hold public assemblies featuring debates on important topics). But his proposal to change the focus of the curriculum from the basic academic subjects to the "power" of "public discourse" (p. 200) significantly privileges form over substance. An emphasis on the study of rhetoric could help to restore the good name of that art, but not when its goal is the tame one of self-analysis, rather than the enlivening one of truth.

Despite Steiner's efforts to temper it, the doctrine of "man as measure" would still operate as a dogmatic relativism at the root of democratic education. It is a misleading dogma, a too-simple response to several fundamental questions of political philosophy, such as the question of natural right. Guided by this theory, teachers and students would hardly be led to ask those questions, much less to comprehend them in their difficulty with the full awareness that a liberal education requires.

These criticisms notwithstanding, *Rethinking Democratic Education* is an important book. It makes a challenging contribution to the debate about democratic education not only by synthesizing potent philosophical tenets but also by frankly suggesting that any project such as this one raises questions about its ability to be put into practice that cannot be easily answered (pp. 68, 119, 189–91, 211). Future scholars of democratic theory will need to confront Steiner's claims.

University of Nevada, Las Vegas DAVID FOTT

Making Citizens: Rousseau's Political Theory of Culture. By Zev M. Trachtenberg. New York: Routledge, 1993. 307p. $59.95.

Considering how often rational choice theorists have used Rousseau as an example of everything that is wrong with democratic theory, Zev M. Trachtenberg's *Making Citizens: Rousseau's Political Theory of Culture* is a surprising book. Indeed, Trachtenberg's book could serve as a primer on Rousseau's thought for those convinced rational choice theorists who believe Rousseau is not worthy of serious study. In several chapters, Trachtenberg shows how some of Rousseau's more opaque pronouncements can be explicated in a formal and systematic way. The core of the book, however, relies upon close readings to analyze Rousseau's thought, particularly his ideas about the political importance of culture. On the basis of these two different approaches, Trachtenberg concludes that Rousseau's theory of culture undermines a people's ability to formulate a general will and therefore frustrates Rousseau's ambition to explain how a functioning political order may be legitimate.

Rousseau's project, Trachtenberg argues, is to solve "the fundamental problem of politics": How are people to retain their autonomy once they submit to the authority of others? The general will—the idea for which he has so often been dismissed—is the solution Rousseau offers to this problem. Instead of being impatient with the vagueness of Rousseau's own account of the general will, Trachtenberg meticulously explains the various reasons one can extract from it for why a community might fail either to formulate or enforce the general will. The analysis here draws heavily on rational choice theory, especially on Mancur Olson's *Logic of Collective Action* (1971). Trachtenberg shows that Rousseau had some conception of public goods, as well as of how free-riding might jeopardize their provision.

Rousseau's political theory of culture proves crucial, Trachtenberg claims, because it is culture that explains why communities fail either to formulate or to enforce a general will. A particular kind of culture ensures what Trachtenberg calls "political success"—a community's being able to arrive at, and act upon, a general will without violating the autonomy of any of its members. To illustrate how politically important Rousseau thought culture was, Trachtenberg compares Rousseau's critique of "society as it is" with his representations of "society as it could be," arguing that the differences between them are grounded in differences between their *moeurs* (cultures). Culture makes or breaks political success.

Rousseau's vision of a politically successful community in *The Social Contract* has often been criticized as unattainable. Trachtenberg deepens that criticism by charging that Rousseau's conception of political success is unattainable not because its standards are too high but because they are incoherent. Important as Rousseau believes culture to be, his conception of an ideal political culture solves the problem of *enforcing* the general will (i.e., how to avoid free-riding) only at the cost of making the problem of *formulating* the general will intractable. Rousseau's vision of a healthy political culture, Trachtenberg claims, relies so heavily on fostering citizens' dependence upon one another that they are no longer independent enough to arrive at the general will in the first place. Both healthy and corrupt *moeurs* stifle autonomy.

To call Rousseau's account of political success incoherent seems to place Trachtenberg's assessment of the value of Rousseau's work in the same category as William Riker's dismissal of Rousseau as an incoherent populist (*Liberalism against Populism*, 1982). Yet Trachtenberg shows how Rousseau's conception of the general will can be used to gauge a community's potential for compromise. Nor does Rousseau's conception of the general will violate the logic of Arrow's Paradox, as Trachtenberg ably shows in appendix 2. What is more, just because Rousseau failed to articulate a coherent vision of a healthy political culture, Trachtenberg implies, does not mean it cannot be done.

Although his assessment of Rousseau is more balanced than those of many rational choice theorists, Trachtenberg's critical conclusion follows from rational choice assumptions about the political process. The claim that Rousseau's conception of a healthy political culture undermines the ability of citizens to arrive at a general will depends on assuming that Rousseau believes that people formulate their ideas of the general will by themselves out of their own conceptions of welfare and that a vote then decides the matter. Trachtenberg points to passages in *The Social Contract* that might support such an individualized view but passes over their ambiguity too quickly. Vague though Rousseau may be on this point, if what the people do together in assembly goes beyond voting, then Trachtenberg's worries about how political culture affects people's ability to come up with a general will seem less persuasive. Nevertheless, this is a stimulating and well-conceived book.

University of Nevada, Reno EMILY HAUPTMANN

Acts of Hope: Creating Authority in Literature, Law, and Politics. By James Boyd White. Chicago: University of Chicago Press, 1994. 322p. $34.95.

We live our lives surrounded by—enmeshed in—authorities that, for the most part, we do not question. These authorities—parents, friends, teachers, bosses, police officers, customs and traditions, laws, churches, and so on—lay claims upon our lives and set directions for our conduct that we ordinarily follow with little thought or question. But sometimes we do challenge the claims made upon us; and at those moments, authority becomes the subject of conscious thought and argument. Such moments can be intensely dramatic. They can be dangerous both for the challenger and for the institution or practice being challenged. This is a book about such moments. It is a book about what happens when authority comes under challenge.

In that respect, *Acts of Hope* occupies familiar terrain, for authority is one of the oldest and most basic concerns of political theory and political science. But James Boyd White's way of exploring that terrain is not the way favored by and familiar to political theorists, especially in modern times. Boyd's mode is rhetorical, literary, dialogical. The theorists' mode is analytical, propositional, categorical. The theorist states a thesis in propositional form, supports it by argument and evidence, carefully stipulates the definitions of important terms, and speaks always in an authoritative (if not authoritarian) tone—the tone of certainty. White offers us not logical calisthenics but conversation, an experience of mind and language not reducible to outline. His words acquire their meanings not by stipulation but through engagement and exchange. In his view, it is ultimately ways of thinking and speaking for which we claim or deny authority, not institutions as objects (the law, the state, the church); and he wants to show us what forms of thought and argument we should respect and how we should go about thinking and talking about such issues. It is this difference between White's approach and the more familiar way of the theorists that gives his book its interest and power.

White has selected some nine texts and episodes as his objects of study. In each of them, either a deeply thoughtful speaker wants to persuade some other or others to think in a certain way about some practice or event, or the speaker is faced with claims that some institution or practice has an authority that overrides the speaker's own desires, judgment, or experience. The speaker engages the practice or audience in a conversation, a conversation in which tone matters as much as content. And as a result of the engagement, both participants are redefined, constituted, and instructed in new ways.

The texts themselves are drawn from different cultural contexts, represent different generic types, and exemplify different modes and grounds of authority. Boyd's range is suggested by the main items and authors: Plato's *Crito*, Shakespeare's *Richard II*, Hooker's Preface to *The Lawes of Ecclesiasticall Polititie*, Hale's "Considerations Touching the Amendment or Alteration of Lawes," *Planned Parenthood* v. *Casey*, Jane Austen's *Mansfield Park*, Emily Dickinson's poetry. Mandela's Speech from the Dock, and Lincoln's Second Inaugural. Rich fare.

In each study, White wants to show how these authors and texts teach us how to judge claims of authority made by others upon us, how to decide when and which institutions and practices are worthy of being accepted as authoritative, and how to create our own authorities through thought and argument. Each study shows how people—concretely, not abstractly—wrestle with the claims of the world upon the self and the claims of the self upon the world.

Acts of Hope is a noble book—graciously written, genuinely thoughtful, deeply intelligent. A brief review such as this can only announce its richness, not specify it. Readers might well argue with some of White's interpretations (I had some trouble with his dismissal of the voice of the *Nomoi* in the *Crito* and with his readings of some of Dickinson's poems); but White invites such arguments, for dialogue is his delight. This book can help us negotiate those dangerous passages when authorities conflict or when they make unjust claims upon us—or we upon them. The book can also lead political theorists out of the thickets of empty analysis and arid abstraction, for it shows how some deeply thoughtful people have actually thought about authority in settings where getting the matter right really matters.

University of California, Santa Cruz JOHN H. SCHAAR

AMERICAN POLITICS

Power and Community: Organizational and Cultural Responses to AIDS. By Dennis Altman. Bristol, PA: Taylor & Francis, 1994. 179p. $54.50 cloth, $19.95 paper.

At last, our discipline has a comprehensive overview of the politics of AIDS that combines the analytical tools of political science with the perspective of an analyst seasoned by a career of writing about gay and lesbian politics. Other social-scientific volumes have dealt with primarily single-nation legal, socioeconomic, and bureaucratic issues that AIDS has either put in the foreground or given rise to; still others have focused on the artistic cultural dimensions of AIDS or, simultaneously, upon its historical uniqueness and its parallels. Dennis Altman's book (despite the subtitle) places the entire duration of the worldwide pandemic within a coherent political framework. As such—and written as clearly as it is—the book will become indispensable as a learned introduction to the topic for political scientists and as a textbook for a variety of political science courses.

One of Altman's goals is to elaborate a theory of the role of human agency, through the medium of "civil society" and, particularly, *community-based organizations* (CBOs), in the shaping of AIDS policy of the nation-state. Through connecting AIDS as a biological phenomenon of poverty and dependence with processes of economic and political "globalization," he illuminates the policymaking capacities of the state in the context of pressures from the dual levels of international organizations and CBOs. Community-based organizations, representing communities defined by behavior (or identity) and/or by place are his basic unit of analysis. Although he calls what he is analyzing the *political economy* of AIDS, his methodology is solidly within political sociology and contemporary democratic theory.

After two theoretical and methodological chapters, the argument moves on to the historically specific question of why a public health response to AIDS necessitated the emergence of CBOs. His argument here dovetails with recent feminist work on an "ethic of care" and shows how such an ethic is articulated institutionally through AIDS CBOs. The book traces the discontinuous progression from an epidemic to a pandemic and the corresponding growth of an activist network through CBOs across "identity-based" communities, as well as internationally across diverse cultures and disparate levels of economic well-being. This development is paired analytically with the evolution of the community sector itself "from grass roots to business suits" (in AIDS writer Cindy Patton's words). As CBOs become integral to the public health capabilities of the nation-state, how are we to conceptualize the *public* in public policy made through a consortium of what has been termed a "pharmaceutical-medical complex" (including clinical practice), state agencies, and AIDS CBOs? Altman demonstrates how this has been answered in Australia, through a case study focusing on the protection of individual rights and representation of affected communities in AIDS policymaking. A chapter on the politics of knowledge and professional expertise about AIDS shows how the privileging of Western biomedicine and social science assumptions tends to reinforce, transnationally, the pharmaceutical-medical complex with its own elite and the political tensions this creates. The book ends on this theme, projecting forward the global political economy of AIDS with the potential "subversive" role of CBOs in (hopefully) assuring some democratic accountability and promoting distributive justice in access to the means of health.

It is here, in a conclusion in which Altman identifies CBOs as "subversive of the existing order," that the argument becomes slippery. He qualifies his assertion by stating, "Community organizations need walk a fine line between seeking to influence the agenda and performance of governments and becoming subsumed into the interests of the state" (p. 162). But more problematic, although AIDS public health has been a vehicle for wresting some control over state policymaking processes by differently disenfranchised groups (gay men, poor racial/ethnic minorities, poor women), it may be doubted how extensive or lasting this can be, as long as the disease is epidemiologically limited to these subpopulations, not reaching the so-called "general population" (as has occurred in the U.S. and other developed countries)? Further, instead of presupposing a rather essentialist teleology of CBOs (as Altman does), can we not reassert popular control despite the tendencies toward cooptation and depoliticization intrinsic to the success of CBOs? More consideration might also have been given to CBOs' role in how the overall parameters according to which health policy is made are being transformed, in part (as AIDS has demonstrated) due to the historical development that health care and health-related policy become increasingly more central to de facto modern governance.

Basic to all these issues is an undertheorization of the relationship between the *political society* as the realm of coercion and *civil society* as that of noncoercion, within which CBOs arise to challenge the terms of coercion. The theoretical deficit results from an overly narrow view of coercion, in which, say, religious organizations even in their shaping of individual conscience or subjectivity would be subsumed within civil society because they use methods of coercion other than those defined as "political." (For example, forms of "civil" coercion would include not only the U.S. religious right's explicit homophobia but institutionalized heterosexism ranging from educational curricula, to employee benefit packages, to popular culture.) The book's Gramscian starting point (never fully developed anyway) preempts a more theoretically nuanced analysis. By contrast, a different theoretical move might analyze how a "response" to "AIDS" involved a redefinition of relationships within (civil) society as health-significant, making them newly accessible to coercive (political) regulation. Such a move could provide us with an understanding of why formerly nonpolitical aspects of social life are recoded as political and vice versa, illuminating cultural and temporal variations both in how the meaning of the political is constructed and what possibilities there are for democratically-minded action.

Aside from these minor reservations, I believe *Power and Community* to be one of the best analyses of the politics of AIDS yet published. Altman's trajectory charts the way toward the future for lesbian and gay political analysis, as well as for all political scientists who want to make sense of the transformations of our political world occasioned by sexuality and AIDS as objects of public policy.

City University of New York, La Guardia MARK BLASIUS

Unequal Struggle: Class, Gender, Race, and Power in the U.S. Congress. By John C. Berg. Boulder: Westview, 1994. 187p. $59.00 cloth, $19.95 paper.

Since the fall of the Soviet Union, Americans have come to believe that communism is dead and that Marx has been proved wrong. It is in this climate that Berg takes a bold step and argues that Congress may best be understood within a Marxist framework. Perhaps unfairly, to be persuasive in such a climate, such an argument must be very strong. While Berg does provide some interesting insights, I doubt the book will convince very many congressional scholars that Marxism provides the best way to understand Congress. Having said that, I must note that I read the book as a congressional scholar and not a student of Marx.

Berg is correct in noting the theoretical weakness in congressional literature. Relying on what he calls pure forms of pluralism and institutionalism, Berg gives examples of how each fails to explain certain actions taken by Congress. However, Berg's point that Marxism provides the needed theoretical framework to understand Congress has its own weaknesses. He suggests that even without getting too specific about which version of Marx one relies on, the basic ideas of "mode of production, class exploitation, and economic determination in the last instance—may yet prove to be better guides to understanding politics and society" (p. 21). Yet by spending considerable energy explaining key terms and discussing their various interpretations, Berg leaves the reader wondering how helpful Marxism is if the terminology is so open to interpretation.

The strongest part of the book is his discussion of the relationship between Congress and class. Congress "looks out for the interests of wealth not only as a result of bribery and campaign spending but also because the economic structure of our society requires it" (p. 32). Giant corporations have hegemonic powers because members of Congress and society see their interests as our interests. He says this hegemony can be seen in the United States' basic legal order (corporations are guaranteed basic rights); separation of powers (the president having greater power in areas important to giant corporation, Congress providing more representation to nonhegemonic powers); and the internal structures of Congress (key committees and leadership positions being dominated by "members who have incorporated the hegemonic ideology") (p. 42). Although one may argue that *hegemony* is too strong a word and that each of these points needs greater clarification, this does provide a new angle to think about policymaking.

It should be noted that Berg does not suggest that all businesses have hegemonic powers. Besides giant corporations, the competitive sector or power bloc (smaller businesses) make up the other part of the capitalist class. This group cannot dominate but is able to win on some key issues. Within this discussion, Berg provides an interesting interpretation of committees. He suggests that what is equivalent of Fenno's *power committees* represent the hegemonic power of the giant corporations and that the members of these committees are free to act independent of constituent's interests to support these businesses. On the other hand, members of policy committees (nonhegemonic committees) must support their constituents' interests.

While wealth is advantaged by the system, labor is not only disadvantaged by economic interests but must support economic interests. By using examples, Berg suggests that labor in the United States is weaker than we would expect from the resources available to it. Although labor can win some victories in Congress, the rules of the game are such that it does not win in the long run. While Berg may underestimate the gains made by labor, he does provide some evidence that labor has not been as successful in the United States as one might think.

While the discussion of class is fairly classical Marxism, the rest of the book seems a little more strained, not fitting Marx quite as well. Since there are questions about how Marxism ought to deal with race and gender, Berg spends much time developing his views on the matter and concludes that gender and race are similar to class because they are all "defined by a particular relation to the social division of labor" (p. 72). Since there are questions as to the place of these groups within Marxism, this discussion weakens the main point of the book, namely, that Marxism can be used to understand Congress. What also raises questions about the place of these groups within Marxism is that the nature of Berg's discussion changes as he goes from class to race and gender. While Berg's analysis of Congress and class focused on the policy successes of the different classes, his discussion of women focuses on the ability of women to gain office, and his discussion of race looks at the behavior of African Americans in office.

In the last chapter, Berg provides an interesting discussion of what needs to be done. The history of disadvantaged groups and Congress suggests that even if members of disadvantaged groups get into leadership positions, they are not able to accomplish much. While working within Congress can help a cause, without social movements to support such work, the economic and political structures will make changes impossible. Although these ideas are not new, they fit within Berg's framework.

The book does not fit in with the dominant body of congressional research—which is one of its strengths. The book contributed more to my understanding of social movements and what they need to do than to my understanding of Congress. While the book mentions how Marxism could explain aspects of the committee system, it does not fully explain congressional activity and organization. I was often frustrated because Berg seemed to be explaining American politics more than Congress. Almost completely left out of discussion is party leadership and strength.

While examining Congress through Marxism does provide some interesting insights, the book is unlikely to convince many readers that Marxism is the best way to understand Congress. Berg's critique of the other approaches is well taken, and he does provide a unique interpretation of congressional policymaking. However, Marxism, as presented here, fails to provided a better alternative to understanding Congress than the other theories. The strengths of the book are the earlier sections, where Berg discusses the economic hegemony on the part of giant corporations and the strength of the power bloc; but he tries to do too much with his discussion of Congress and race and gender. Perhaps a bigger problem is that Marxism would fail to explain all congressional behavior. I am not sure how economic determinism, class, or modes of production would explain policies such as school prayer, term limits, or gays in the military.

Oklahoma State University REBEKAH HERRICK

Trust: Representatives and Constituents. By William T. Bianco. Ann Arbor: University of Michigan Press, 1994. 216p. $39.50 cloth, $15.95 paper.

Democracy requires that constituents evaluate their representative's behavior. Such evaluations are necessary for the election of representatives responsible to constituent interests. These evaluations are complicated, however, by voters' ignorance of key elements of the political environment. For example, constituents may have limited information regarding the policies that best promote their interests. Constituents may also have little knowledge of the legislative process and, therefore, the relationship between their representative's legislative behavior (e.g., voting, coalition building, etc.) and the disposition of candidate policies. Such uncertainties can also give rise to *trust*—favorable assessments by constituents of their representatives that are independent of the representative's voting behavior. In this interesting book, William T. Bianco explores the foundations of trust in the relationships between constituents and representatives and, along the way, contributes some novel interpretations and determinants of congressional voting behavior.

Bianco's chief theoretical insights are gained through the construction and analysis of a two-person, noncooperative game under incomplete information. The actors in this game are a constituent and his representative. The representative has superior information about the effects of some legislative proposal and the congruity of her interests with the constituent. The representative votes on the proposal and the constituent evaluates this behavior for electoral purposes. Trust, or *voting leeway*, occurs when the constituent gives a favorable evaluation to the representative (i.e. votes for reelection) regardless her voting behavior. The frequency of trust is shown to increase both with the degree of constituent uncertainty about the consequences of the legislative proposal and the prior likelihood of common interest between the constituent and his representative. Trust, then, is a predictable feature of the political environment engulfing the representative process.

Bianco explores the external validity of this leeway hypothesis by interviewing representatives regarding their behavior on two key votes in the 101st Congress: the Ethics Act of 1989 (a major feature of which was congressional pay raises) and the repeal of the Medicare Catastrophic Coverage Act. In each case, the variable of interest is the representative's response to a question regarding the intensity of constituents' views on the vote; that is, were constituent opinions so developed that a representative voting at variance with these wishes would suffer a negative evaluation, or were constituents willing to trust the judgment of their elective representatives? Bianco then constructs a sequence of contingency tables to show that (1) trust (voting leeway) was an important element to the passage of at least one of these bills, (2) at least for nonmarginal legislators, personal characteristics (e.g., personal wealth, honoraria, and outside income) were correlates of trust, and (3) postvote explanations of votes inconsistent with voter opinion are made by representatives who report little voting leeway. Bianco interprets these findings as broadly consistent with the leeway hypothesis (and a corollary referred to as the *explanation hypothesis*) and as suggesting that far from being an overarching concept, trust between a representative and her constituents is context-specific and is built or dissipated one bill at a time.

There is much to recommend this book. Scholars of congressional voting behavior will value Bianco's attempts to place his findings and perspectives within the extant literature. Indeed, chapters 2 and 3 contain a broad and novel survey that makes Bianco's work fit quite productively within the literature. Scholars of positive and quantitative political theory will appreciate the marriage of a solid theoretical analysis with a descriptively rich empirical inquiry. In this regard, Bianco's analysis is firmly in the tradition of previous books in this interesting series published by the University of Michigan Press. This would be a stimulating book for a graduate seminar in congressional studies.

The chief weakness of the book is the empirical analysis in support of the theory. To be sure, there is nothing inherently limiting in either the use of survey-response techniques or the paucity of cases evaluated; both are longstanding traditions in the field of congressional studies. However, the presentation of the data compiled for each case is undeveloped. Why, for instance, is the data presented solely in standard contingency tables when multivariate statistical techniques appear appropriate (i.e., probit analyses)? Why not directly estimate the determinants of perceived trust as a function of the other variables introduced in the analysis and, in the process, gain an appreciation for the statistical validity of the relationships? Bianco might enhance the value of this evidence by subjecting it to more rigorous statistical methods.

As with any serious book, there are many nits to pick. Bianco's criticisms of the extant literature based on his analysis are, in my view, both too broad and undeveloped. For instance, Bianco cautions against the use of an induced ideal point (one based either on constituent characteristics or legislator preferences) as the sole predictor of legislator behavior (p. 91). Recall that one of Bianco's chief findings is that perceived trust also bears on voting behavior. Bianco's point, which is correct and well known, is that given any fixed ideal point, a legislator's behavior (voting or otherwise) can be different depending on whether information is complete or incomplete. One thing we would like to know is how different these behaviors can be. How important is incomplete information for explaining variation in congressional voting behavior? Until this basic question is answered, criticisms of complete information models and methods based on the likelihood of incomplete information are statistically impotent.

Notwithstanding these criticisms, Bianco's research has value in its explicit development of an analysis of the relationship between a constituent and his representative. This relationship is tempered by the very real characteristics of the imperfect monitoring of representatives and incomplete information about policy outcomes. There is much more to be learned by further analyzing such models. Bianco's book is an important initial effort that contributes to our understanding of congressional voting behavior.

University of Southern California THOMAS W. GILLIGAN

Gay and Lesbian Politics: Sexuality and the Emergence of a New Ethic. By Mark Blasius. Philadelphia: Temple University Press, 1994. 239p. $24.95 cloth, $14.95 paper.

Blasius' book on gay and lesbian politics makes two principle contributions to the contemporary discourse on the politics of the gay and lesbian movement. First, Blasius contends that lesbian and gay existence is best conceptualized as an *ethos,* as opposed to a sexual orientation, sexual preference, life style, subculture, or community. Blasius argues that unlike the terms most frequently ascribed to gay and lesbian existence, a lesbian and gay ethos implies a conscious repudiation of society's dominant regime of truth, *compulsory heterosexuality* (a term borrowed from Adrienne Rich's 1980 article in *Signs*).

Sexual orientation is problematic because of its association with the medical model of homosexuality, the cornerstone of the oppressive pathological etiologies of gay and lesbian existence. *Sexual preference,* on the other hand, reproduces the dominant regime of truth by trivializing the differences between gays and straights to anatomical proclivities or preferences. If this were indeed an accurate representation of public opinion, he argues, there would be no need for the institutions developed by lesbians and gays to secure their safety and promote their well being.

Life style, on the other hand, implies a degree of self-reflection, self-creation, and community building, qualities that Blasius feels are the core of an ethos of lesbian and gay existence. But life style "is also a principle site at which we have all come to be governed" (p. 191). Life style, for example, has come to be associated with consumerism with its attendant materialistic assumptions concerning individualism, self-worth, and social obligation. The idea that gay and lesbian existence can be conceptualized as a life style ultimately suggests that gay and lesbian desire conforms, like all other life styles, to hegemonic social norms and power relationships. Thus, he argues that although *life style* incorporates some aspects of *ethos,* it is ultimately less encompassing and satisfying.

The idea of a lesbian and gay *subculture* or *community* is clearly much closer to the author's own ideas concerning an ethos: "What is most important about it [is] that this sociologic community does facilitate the creation of . . . moral agency, . . . the creation of a specific kind of subjectivity" (p. 202). "Coming out" is perceived as an aspect of self-knowledge; socially, as an assertion of rights; and as creatively and intellectually constructing an alternative to compulsory heterosexuality as an order of truth about gays and lesbians. This specific kind of subjectivity is fostered by community, but it is enacted beyond the confines of the gay and lesbian subculture or community. The idea of a lesbian and gay ethos therefore constitutes an implicit challenge to the hegemony of compulsory heterosexuality.

A second contribution of *Gay and Lesbian Politics* is the author's insightful discussion of gay and lesbian rights. Rejecting the proposition that the subject can be reduced simply to the current imbroglio between lesbians, gays, and the state, Blasius argues instead for an understanding of rights as relational: "It goes beyond a juridico-discursive model of rights as granted to or withheld from individuals by the state to a conceptualization of a right within a normalizing-disciplinary ordering of power" (p. 133). This conception of rights puts the significance of social, economic, and religious institutions for lesbian and gay rights in the foreground. The implication of Blasius' argument is that the regulatory practices of the medical establishment, for example, can be as significant for gays and lesbians as the regulatory practices of the state. His discussion of AIDS and *biopower* is particularly apropos.

Relational rights are predicated on the assumption that individuals desire and need the freedom "to constitute themselves as ethical beings" through choosing their "sexual-affectional relationships with others" (p. 141). For gays and lesbians, this means establishing their right to love whom they choose in a manner consistent with the mutual desires of the parties involved. A relational right, therefore, is based on the right of sexual-affectional self-determination. It is, furthermore, an assertion of the social and political validity of such a right and a demand for distributive justice. As such, the gay and lesbian ethos necessarily politicizes the normative order of compulsory heterosexuality, a normative order traditionally validated by metaphysical appeals to "nature" and "rationality."

Blasius' perspective on rights and his articulation of a gay and lesbian ethos are informed by his careful reading of Foucault's concept of biopower, a concept that at one and the same time, calls into question the efficacy of the juridical-sovereign concept of power and asserts that modern power is, as well, an "administration of bodies and the calculated management of life" (p. 60). Blasius succeeds in explaining the implications of biopower for understanding sexuality as a "technology of government," the relevance of this concept for understanding gay and lesbian existence, and its implications for reconceptualizing the political goals of the lesbian and gay community.

Blasius is most provocative in his discussion of *erotics* as the "principal ethical category for lesbians and gay men," arguing that it also "transforms sexuality as a technology of government" (p. 121). While there is little doubt in my mind that such erotic techniques are instrumental in challenging the dominant regime of truth about sexuality, the centrality of erotics in Blasius' theory of ethical agency (as opposed to other dimensions of gay and lesbian existence) is certain to be a point of contention between lesbians and gay men.

Aside from the centrality of erotics in the development of the ethical agency of gays and lesbians, Blasius overly idealizes the ethical implications of gay and lesbian sexuality (what he chooses to call "erotic friendships") in his explication of relational rights. This is particularly apparent in his assumption that the self-conscious role-playing so characteristic of some relationships is a bridge to equality in the wider social and political arena.

Nevertheless, this is a highly rewarding work. It breaks new ground in the author's discussion of what it means to be lesbian or gay in a culture that has historically rendered gays and lesbians "socially dead," in his redefinition of gay and lesbian rights, and in the implications of this analysis of gay and lesbian politics for our understanding of the political.

Ohio University RONALD J. HUNT

The Presidential Pulse of Congressional Elections. By James E. Campbell. Lexington: University Press of Kentucky, 1993. 273p. $36.00.

The Presidential Pulse of Congressional Elections brings together in a single volume ideas and data from articles that James Campbell has published in midterm elections, adds some new data, and presents a comprehensive surge-and-decline model of congressional elections. It has a problem, however: events have given him facts that do not fit his story.

Campbell ends with an epilogue on the slightly anomalous 1992 election. According to the basic model, Clinton's win should have been accompanied by a pickup of seats by the Democrats, not by the losses that occurred; but it is a minor deviation. As Campbell points out, four of nine presidential candidates in this century who won with less than 55% of the vote have seen their party lose seats. The missing surge is just one of those "spikes" in the record, disproving nothing. The 1994 result is another matter. It is outside of Campbell's story. A surge-and-decline analysis of the 1992/1994 pair, based on his equations, predicted a Democratic loss of 18 or 19 seats (Table 6.2); they lost 52.

However, this obvious miss does not detract from Campbell's contribution. If we are to account for 1994-like events, we need to understand the typical midterm. And the typical midterm, as Campbell sees it (and I agree), is substantially shaped by the preceding presidential election. With both data and citations, Campbell establishes the need for a surge-and-decline model of midterm elections and then proceeds to provide it.

The centerpiece of the story is short-term forces creating three groups of voters: (1) nonpartisans and political marginals, (2) partisans whose preference is reinforced, and (3) partisans who are cross-pressured. Independents and swing voters are strongly attracted to the winning party and all its candidates; partisans of the winning presidential candidate's party are excited and turnout at above-average rates; partisans of the loosing candidate are discouraged and tend to stay home. The presidential election is a sum of these behaviors, over each of the groups. The effect trickles down the electoral structure, providing an advantage to the candidates of the winning party and, in the event, electing a disproportionate number of them. In the midterm, the winning presidential candidate's appeal is absent; and without it, lower-level candidates of the winner's party, ceteris paribus, do less well. As Campbell notes, the lessened advantage can quickly snowball into a substantial disadvantage. The sitting president can incur significant disapproval for both his success and failures. (Even when the president wins a battle, he makes enemies of those he defeats; and, as we know, a desire among the aggrieved to punish seems more mobilizing than a desire to reward.) Further, an unpromising midterm environment can induce thoughtful and ambitious politicians to sit out the election, and the sitting president's party can field a weak crop of candidates. The net result of this elite and voter behavior is the off-year decline. It does not *have* to be a decline, but the missing stimulation of the presidential surge added to the grievances created by the president's policies and programs usually result in off-year losses.

Campbell's data generally support the study. The pattern of surge and decline is nicely documented as a national effect, within the congressional districts, and for lower-level offices. The evidence for the hypothesized behavior of voters come through less clearly. The basic patterns are consistent, but the manner in which the data are presented (everything is done with regressions) does not make it easy to see magnitudes and differences. Some straightforward cross tabulations, averages, or graphs might have yielded more memorable data. In fact, there were several facets of the analysis that I found troubling. The regressions are usually simple linear forms, yet anyone who has worked with these data knows that the relationships are not always as simple as the ordinary least squares forms that are the universal technique in this book. I have found considerable curvilinearity in the relationship between presidential year surges and midterm declines. The simple linear estimates in the book seem, from my results, to misestimate the magnitude of the slope and the strength of the surge-decline relationship. Also, a path analysis estimating the direct and indirect effects of the surge and attempting to identify candidates affected by it (chap. 7) seemed problematic both for the calculation of the path effect and the percentage point deviations in the districts.

On the whole, however, the book has a lot of useful material and Campbell's revised theory of surge-and-decline is persuasive. If we learned anything from 1994, it is that we have not isolated all the variables that shape midterm elections. The state of the economy and the preceding presidential election are only a few of the multiple determinants. *The Presidential Pulse of Congressional Elections* will help to formulate the research that will get us closer to a story that deals with elections like 1994.

University of California, Los Angeles JOHN R. PETROCIK

The Regulated Economy: A Historical Approach to Political Economy. Edited by Claudia Goldin and Gary D. Libecap. Chicago: University of Chicago Press, 1994. 312p. $56.00.

Culled from a 1992 National Bureau of Economic Research conference, this volume contains seven interesting historical case studies authored by talented political economists studying late nineteenth- and early twentieth-century regulation. The result should engage students of regulation, organizations, and the analysis of political development and state building.

For those concerned with political development and state building, the individual contributions illustrate nicely how the political economic approach contrasts with that popular among students of state building. In general, the authors of *The Regulated Economy* pick topics that are narrower empirically (e.g., orange-marketing orders, the regulation of utilities in Chicago) and are less temporally expansive than their counterparts. The subjects of analysis also more often appear to be chosen to understand a precise theory than to explain a political change of fundamental importance. One obvious critique may be that the topics selected are sometimes so minor or unique that they are inappropriate for producing theoretical insights. Obviously, this issue is beyond the purview of a brief review.

The Regulated Economy has somewhat different implications for students of regulation and organization. In their introductory comments, the editors assert that all the authors attempt to "examine the ways constituent groups emerged and demanded government action to solve perceived economic problems" (p. 1). Yet when the contributions are analyzed in their totality, the enduring theme generated is that things are complicated (or that individual cases have idiosyncratic components). A wide selection of factors undermine mechanistic applications of Chicago-style models of the Stigler/Peltzman/Becker variety. Most notably, only one of the papers, Claudia Goldin's investigation of why immigration laws became more restrictive with time from 1890 to 1921, seems largely consistent with such an approach. While she explains change by examining the activities of organized labor, business, rural America, and immigrants themselves, all of the other examinations highlight obstacles to such straightforward economic and demographic applications. Institutional arrangements, long-term ideology, concerns about the public interest, technological change, vote trading, commitment problems, state fiscal concerns, political entrepreneurs, industry cohesiveness, and coalition formation are all posited to structure or hinder group influence. The implicit agenda that emerges revolves around a need to specify the relative importance of this litany of factors and the detailing of the conditions when such features are germane or inconsequential.

Thus, a variety of explicitly political characteristics are found to undermine or mediate interest articulation. For instance, in analyzing efforts to regulate the rails in Illinois in 1870, Kanazawa and Noll demonstrate how popular and constitutional-convention voting yield different results when alternative theories of regulation are tested. In an example that is particularly profound, given the plethora of similar modern-day experiments, they find that referendum voting is well explained by economic measures but that such factors are far less relevant for making sense of ballots cast by representatives.

Similarly, in their analysis of nineteenth-century banking regulation, Wallis, Sylla, and Legler claim that it is the state's fiscal interests that mitigate group influence. They convincingly demonstrate that at least for the case studied, regulation may be endogenous to methods of taxation, and vice versa.

Analogously, Calomiris and White cite characteristics such as political entrepreneurship (a factor frequently discussed by political scientists) as crucial. They find that states with more vulnerable banking systems pushed for nationalization of federal deposit insurance in accord with their economic interests. However, they also maintain that with the assistance of entrepreneurs, federal deposit insurance left the sphere of special interest politics and entered the larger public domain in the 1930s, when deposit-insurance finally became a reality. (The obvious alternative explanation—that high political turnover produced a different group of key decision makers—might have been explored in more detail.)

In a similar vein, Poole and Rosenthal find that economic interests were "filtered and redirected" (p. 116) in congressional fights over railroad regulation in the late nineteenth century due to the long-run ideology of legislators and the orchestration of vote trading by political parties. By convincingly showing that a simple spatial model explains voting patterns, they imply that groups will have a difficult time in guiding legislator behavior in ways that might create significant tensions with ideological and partisan dispositions.

With respect to organizations themselves, both Hoffman and Libecap and Kantor and Fishback find that difficulties with maintaining cohesiveness, even among interests that would seem to be natural allies, undermined group influence. Thus, according to Hoffman and Libecap, orange cartelization failed to raise prices for Florida and California producers during the Depression largely because of difficulties with forging agreement among industry participants despite government efforts to assist (although the issue of why some industries with similar structures may be cohesive is not broached). Similarly, Kantor and Fishback suggest that since uniform support among even narrow interest groups cannot be taken for granted, when and whether majority coalitions can be assembled to guide public policy are likely to be key. Along these lines, they find that fights over workers' compensation in Missouri from 1911 to 1926 influenced not just the passage of legislation but the specifics of the package adopted.

Finally, there may be mitigating factors that are largely exogenous. Troesken, for instance, maintains that regulation of Chicago's manufactured coal gas industry was heavily structured by a variety of such forces, most notably technological change. Consequently, he maintains that the interaction of technological development with political factors led to state utility regulation in the early twentieth century, rather than a reliance upon market forces or municipal regulation (although one might question whether the claim that municipal regulation was really much more in the public interest is substantiated).

In short, *The Regulated Economy* is a fine representation of modern political economy as applied to historical issues of regulation that is both worth reading and quite engaging. Particularly for those interested in regulation and interest groups, the challenge it presents is how to develop models of organizational formation and interest articulation that are parsimonious and powerful. However, this only suggests that a good deal more interesting work remains.

University of Rochester LAWRENCE S. ROTHENBERG

The Peace Progressives and American Foreign Relations. By Robert David Johnson. Cambridge: Harvard University Press, 1995. 448p. $45.00.

In this well documented and nuanced account of American foreign policy in the early years of this century, Robert David Johnson calls for a renewed appreciation of the role played by the Senate "peace progressives" in articulating a liberal alternative to Wilsonian foreign policy. The Senators who comprised the peace progressive bloc opposed what they perceived to be the business-driven, exploitative international relations of the 1910s and 1920s. They favored, instead, an antiimperialist, though internationalist, foreign policy in which the United States would use its economic and moral influence to support the right of self-determination and peace. Johnson documents the evolution of the peace progressive ideology from its intellectual roots in Paine, the Anti-Federalists, Jefferson and William Jennings Bryan, through its heyday of influence in the 1920s and ultimate decline and transformation into isolationism after the Depression. Johnson argues that the peace progressive philosophy was not merely a mutation of Wilsonian internationalism but "a powerful and sustained left-wing dissention" (p. 315). Combining disarmament and antiimperialism with elements of American moral, diplomatic, and economic power, the peace progressives articulated a global vision that served as counterpoint to the foreign policies of the Taft, Wilson, Harding, and Coolidge administrations. Johnson's work crafts a solid foundation for understanding the original (if often unrealistic) peace progressive ideology as an opposition force to Wilsonianism and the business-dominated internationalism of the 1920s.

A major strength of Johnson's work is that it does not overplay the power or influence of the peace progressives. In his meticulous accounting of the evolution of the movement, we see the difficult and at times unstable coalition of diverse, strong-willed senators. The cast included, over a period of time, such figures as Asle Gronna, William Kenyon, Robert La Follette, John Blaine, Henrik Shipstead, William Borah, George Norris, Gerald Nye, and Burton Wheeler, among others. The fundamental consensus underlying the coalition was an alienation from the traditional power politics of Europe and an affirmation of the rights of weaker states to self-determination. The most coherent formulation of the philosophy came in opposition to U.S. intervention in Latin America and the Caribbean, though the peace progressives also were vocal in policy toward Asia, the Soviet Union, and post–World War I Europe. The peace progressives were internationalist in outlook but believed that the vast economic resources and moral suasion of the United States should be used to press European powers to reduce their armaments and imperialist standing, rather than to enrich the coffers of international business and reinforce the position of dominant world powers. Their success in opposing U.S. membership in the League of Nations came as a result of a marriage of convenience with the Irreconcilables, each opposing Wilson's stubborn inflexibility against reservations in the charter, rather than as a result of the mere force of their arguments. Even at the height of their influence in the 1920s, Johnson is careful to recognize that the success of peace progressives' efforts (e.g., moderating Coolidge's aggressive politics toward Mexico and cutting off funding for the U.S. occupation of Nicaragua) was as much the result of a vacuum left by an internally split Democratic party unable to formulate a coherent alternative to Coolidge's foreign policy as it was to their actual policy positions.

The Depression made the peace progressives' international position increasingly difficult to sustain as the floundering global economy increasingly forced difficult trade-offs between the needs of domestic constituents and the positions of nations they had previously defended. Trade policy became emblematic of the contradictions inherent in seeking nonexploitative international business relations while at the same time protecting the economic interests of domestic constituents. In a finely nuanced discussion, Johnson demonstrates that the peace progressives' transition from supporting internationalist to supporting isolationist policies was, in fact, a logical extension of their ideology, given the international context. Having become disillusioned with Germany and the Soviet Union as potential allies in sustaining an antiimperialist world order, the peace progressives came to view all European powers as malevolent. The effort to distance the United States from traditional European power politics, therefore, led the peace progressives to oppose the World Court and to support the Neutrality Act in 1935. Linking the ideology of the 1910s peace progressives to that of the 1930s coalition, Johnson demonstrates that "this strand of thought, reflecting the peace progressive hostility to what they perceived as an immoral European balance of power system, had always existed in the ideology of the dissenters. With the Great Depression and subsequent changes to the international system, it became far more pronounced" (p. 305).

Political scientists looking for a coherent theory of the dynamics of Senate coalition formation or of the formulation of American foreign policy will not find it in this book. Johnson is a historian; and in the tradition of scholars of diplomatic history, he uses thick description to underpin his basic theme and illustrate the evolution, influence, and demise of the peace progressives. Perhaps the only shortcoming of this book is that Johnson does not capitalize on some of his own evidence to illustrate broader themes and further ground the importance of studying groups such as the peace progressives. Though Johnson is clearly familiar with literature analyzing domestic factors that may have motivated the positions of the Senators who comprised the peace progressive bloc, he argues that such inquiries address the wrong issue: "Asking what 'motivated' the Senate dissenters implies that somehow factors not concerned with international relations . . . predetermined their foreign policy positions. Quite simply, this was not the case" (p. 7). Yet throughout the text, Johnson himself notes numerous instances in which domestic constituencies significantly influenced the stances taken by the various Senators, particularly regarding agricultural or trade policy. Johnson may be underselling his own findings, which actually provide interesting evidence of the varying latitude legislators enjoy in formulating foreign policy positions depending on the relative salience to constituents.

Johnson pays considerable attention to the alliances formed between the peace progressives and the peace movements of the 1920s, yielding mutual benefits in furthering their common agenda. Yet there is little systematic discussion of the broader theoretical question concerning how minority coalitions influence policy outcomes. Still, for political scientists interested in comprehensive theories concerning the role of Congress in foreign policy or the influence of minority coalitions, Johnson's book provides a solid body of detail on one such coalition over a specific period of time. Its focus on a marginally studied coalition fills a gap among the more commonly recognized influences on foreign policy during the interwar period and will be primarily of use to specialists in the field. Johnson is to be applauded for a well-written work of impressive scope and depth.

University of Connecticut MARTHA L. GIBSON

The Federal Civil Service System and the Problem of Bureaucracy: The Economics and Politics of Institutional Change. By Ronald N. Johnson and Gary D. Libecap. Chicago: University of Chicago Press, 1994. 229p. $47.00 cloth, $16.50 paper.

Many political scientists find rational choice analysis inaccessible and ahistorical. Economists Ronald Johnson and Gary Libecap show, however, that this need not be the case. Johnson and Libecap effectively combine historical analysis with the tools of rational choice theory and institutional economics to explain the origins and development of the federal civil service system, drawing several challenging conclusions along the way.

Johnson and Libecap are centrally concerned with the "problem" of bureaucracy at the federal level, which they characterize as "a lack of productivity and accountability" (p. 154). The authors stress that the federal bureaucracy is not a monolith; it is segmented into political appointees with policymaking authority, top career officials with at least some policymaking discretion, and the rank-and-file federal workers who do the day-to-day work of the federal government. This latter segment most concerns the authors, for they argue that these individuals can exert considerable influence on public policy. But the development of federal personnel system has left those vast middle ranks insulated from political control and from incentives that could improve their productivity. Johnson and Libecap are ultimately concerned with explaining development and change in the civil service system as an institution and how that bears on economic growth and welfare maximization.

The authors outline their argument simply and clearly in the first chapter, making explicit the argument's conceptual foundations in the public choice literature. They then effectively unfold their argument in detail in the succeeding chapters. Johnson and Libecap argue that the principal common theme in histories of civil service reform—that moral crusaders forced reform on reluctant politicians—is wrong. Instead, they conclude, via qualitative and quantitative analysis, that the passage of the Pendelton Act and the initiation of a merit system for federal civil service employment came about because it served the interests of members of Congress and presidents in two important ways.

First, federal employment had grown too large for a patronage system to remain manageable. Basic requirements for good organizational and managerial control could no longer be met. The size of the patronage workforce thus had to be reduced because it had begun to generate substantial political costs. The authors stress that this was a special concern of presidents, whose national constituencies included key business and professional groups not well served by the poor quality and inefficiency of government services delivered by patronage-dominated federal workers.

Second, limitations on patronage ensured that neither the president nor Congress could gain sufficient control of the federal workforce to gain the upper hand in the perennial political competition between the two branches. Moreover, placing limits on patronage effectively limited its supply, thus raising the value of the patronage exchanges between the president and members of Congress used to reach agreement on policy initiatives.

Once a merit system was in place, it permitted federal employee unions to organize and grow. The unions, largely representing the rank-and-file—and not the upper, policymaking echelons of the career service—became powerful interest groups that exerted the greatest influence on the shape of the federal personnel system, including the debilitating characteristics the authors claim are familiar today: salary levels for the rank-and-file that exceed private-sector counterparts, lack of managerial flexibility in hiring and firing, poor productivity, and unresponsiveness to the public. Johnson and Libecap conclude that the prospects for meaningful institutional change and reform are dim, both because the federal employee unions can effectively wield the mythological horror of patronage as a club against reform and because it is not in the interest of presidents or Congress to alter the relative balance of power that they have achieved over the federal bureaucracy.

This is an important book, well organized and argued. Unfortunately, it is marred by several substantive flaws that detract from its overall impact. The authors simply go too far in discounting the morality dimension in historical accounts of civil service reform. They make a persuasive case that the concerns of vote-maximizing politicians were central to bringing about a merit system; but the historical record is clear that the turn against patronage was not just about management problems, it was also one crest in the wave of moral activism evident throughout American political history. Furthermore, the authors argue effectively that managerial concerns led presidents to seek substantial limitations on patronage, but they offer no evidence that presidents were responding to constituent demands (rather than conceptions of the duties and obligations of the office) in acting on those concerns.

It is also surprising, given the insights on patronage and civil service reform the authors do offer, that they fail to recognize what patronage really was, namely, a system for public funding of election campaigns. By setting the pay of federal workers at a level that allowed politicians to extract political assessments, federal elected officials were simply laundering public funds for their campaigns through government employees. As the authors do note, elected officials had to be sure they could find alternative sources of campaign funds (through interest groups that would eventually include federal employee unions) before they were willing to proceed with limiting patronage.

In their two-chapter analysis of the establishment and growth of the federal employee unions, Johnson and Libecap show that unions have become powerful interest groups whose influence on the shape of the personnel system has generated significant effects on "bureaucratic incentives and the ability to alter policy" (p. 158). But in trying to explain the political success of the unions, the authors also reveal the limits of rational choice analysis of interest group power. All they essentially conclude is that the unions are powerful by virtue of a little voting power, big political action committee sums strategically spent, and lobbying power. This last element consists of spending a lot of time with members of Congress on key committees. None of this is much of a revelation.

Finally, the book lacks any anchor in political theory, particularly with respect to institutional and regime design. In arguing with other economists about whether politics will lead to economically efficient institutional design, the authors make the astonishing statement that "the political process will lead to institutional choices that differ from what one would expect if the notion of neoclassical economic efficiency were the dominating criteria" (p. 179). The possibility that institutions might be designed to meet other criteria seems to escape them. However, in making this argument, the book renders an important service by showing political scientists how important it is to place the wealth of rational-choice-based conceptual arguments and empirical findings in the context of the fundamental questions that liberal-democratic governance poses about the nature and purposes of different political institutions and the contributions they make to a well-functioning regime.

Clark University BRIAN J. COOK

African Americans and the New Policy Consensus: Retreat of the Liberal State? Edited by Marilyn E. Lashley and Melanie Njeri Jackson. Westport, CT: Greenwood, 1994. 246p. $55.00.

To many observers within and outside the African American community, the Reagan-Bush years involved an assault on civil rights gains, an assault that deepened the problem of urban poverty and increased racial tensions. Despite much scholarly concern with how black America has fared under an increasingly conservative policy climate, we have few serious investigations of the sources, nature, and consequences of the apparent "retreat of the liberal state." *African Americans and the New Policy Consensus* takes a few measured steps to fill this gap.

Lashley and Jackson's edited volume is composed of 10 essays. Like most edited volumes, the chapters are of uneven quality. Ironically, two of the three chapters squarely concerned with the overall theme of the volume are among the weakest: chapter 1, which addresses theories of the liberal state, and chapter 4, which addresses how to reclaim access to influence in state policymaking. Although both pursue worthy intellectual aspirations and offer many observations and opinions with which I completely agree, the chapters lack an analytical, theoretical, or evidentiary base required to be truly convincing or insightful.

A number of the other chapters (though typically of narrower scope) are a good deal more successful. The most provocative is Floyd Hayes's discussion of the economic philosophy of the "Age of Reaganism." He reviews studies of black self-help efforts that convincingly refute the assumption that black culture (or the black middle class specifically) has failed to help the black underclass adapt to ongoing structural transformations in the economy. He persuasively addresses what African Americans must begin to do in order to position themselves in the modern age of the "post-industrial managerial state . . . dominated by professional expertise" (p. 110).

Two chapters take up one enduring legacy of the Reagan-Bush years—competing claims and images of black culture as a force in politics and economics. Adolph Reed's chapter is a useful reflection on the current cultural politics agenda among African American intellectuals. He criticizes an insidious racial essentialism and trend toward depoliticized, ahistorical cultural interpretation and celebration on the part of several eminent black scholars. Rhonda Williams offers an interesting assessment of cultural interpretations of black economic and social conditions. She rightly chastises the circularity of most cultural accounts of ethnic group differentials in socioeconomic attainment. However, her work is quickly becoming dated. None of the citations are more recent than 1986, resulting in important omissions in coverage (e.g., both of Larry Mead's books on poverty and Charles Murray's *Losing Ground*).

Other chapters illuminate the limited gains blacks have made against discrimination and in their struggle for a fuller measure of political influence and economic resources. Critical in this regard is William Jackson's careful analysis of data on racial discrimination in home mortgage lending. This paper is a rare and clear-cut demonstration of racial discrimination against blacks that cuts across class lines. Using high-quality data that includes crucial control variables (e.g., credit history), Jackson shows that blacks face direct and significant racial discrimination in obtaining home loans. Claude Barnes's chapter paints a vivid portrait of the utter failure of black electoral dominance in Atlanta to forestall the increasing economic impoverishment and racial residential isolation of much of black Atlanta. Marilyn Lashley maps national trends in educational attainment and income, showing (especially for income) persistent black disadvantage, even after controlling for age, education, and family structure. Cedric Herring, one of our leading analysts of black public opinion, analyzes black-white differences in political disaffection using data from an Illinois survey. He shows that there is considerable agreement in the policy goals of the black public and black elected officials. The problem is the tremendous gap between black collective aspirations and the opinions of the mass white public and white elected officials.

We have entered a period of grave concern and deep questioning of previous assumptions about where the black community stands in the American polity. A number of the essays in Lashley and Jackson's volume, especially those by Floyd Hayes and by William Jackson, shed important new light on how African Americans have fared in the current, far more conservative policy climate.

University of California, Los Angeles LAWRENCE BOBO

Birth Control Politics in the United States, 1916–1945. By Carole R. McCann. Ithaca: Cornell University Press, 1994. 242p. $29.95.

In the fog-shrouded foothills of Santa Cruz, California, lurks one of the most interesting intellectual institutions in the country. The History of Consciousness Program (HistCon to its friends), located at the University of California, is an interdisciplinary program which takes recent postmodernist theory seriously, attempting the daunting task of trying to do empirical work under its gaze. Scholars trained in this program routinely ignore the boundaries separating history from political science, literary studies from sociology, and (as they would put it) those boundaries which try to separate discourse (talk) from practices (action).

Carole McCann's *Birth Control Politics in the United States, 1916–1945* is a quintessential HistCon book, complete with both the virtues and the vices that this new paradigm brings in its wake. Now at the University of Maryland, McCann undertook the original study that is the basis of this book during her graduate stay in Santa Cruz; the intellectual influences are quite apparent.

At its best, this book illustrates the promises of this postmodernism for the social sciences. For example, there is a delicate and critical sensibility focused on the use of language, calling into question the "professional" and "scientific" terms that most scholars, including myself, have used unthinkingly in writing about the politics of contraception and abortion in this country. Making the fundamental HistCon point that language creates, as well as reflects, social reality, McCann examines how *birth control* became *family planning*, shedding its left-wing and feminist roots in the process. Particularly compelling is McCann's examination of how women became defined as "sexually active" and thereby "exposed" to pregnancy, becoming as a consequence "at risk" of getting pregnant. McCann would probably be horrified at the most recent neologism in the field: recent concerns about AIDS have led researchers to write of "vaginal partners" to connote heterosexual sexuality that includes behaviors capable of transmitting a pathogen.

Another particularly praiseworthy part of this book is its careful attention to the class and racial politics that undergirded the "Sangerist" (McCann's term) fight to make contraception available to all women. While the class dimension has received a great deal of attention in the work of previous scholars, including Linda Gordon and Ellen Chesler, and the race dimension has been addressed by Angela Davis, among others, to my knowledge, this is the most careful and detailed account to date of the birth control movement's attempts to include African Americans in the ranks of both the providers and the recipients of birth control.

McCann does a good job of rehabilitating Margaret Sanger from the accusations of earlier scholars that Sanger subscribed to dubious racial and eugenic theories, thereby transforming the birth control movement into a subtle form of social control. With respect to race, for example, Sanger was personally committed to racial equality and fired or transferred workers who were bigoted, and her "annual reports of the clinic's activities . . . were carefully constructed within a discourse of racial equality that contested the dominant ideology of inherent differences between the races" (p. 152). Still, Sanger thought that many African Americans in Harlem were inferior to whites, although she saw this as an effect of poverty, not genes. Sanger also believed that African American women could and would use birth control if given the option, despite the conviction held by many that black men and women were either too irresponsible or too unmotivated to take on the middle-class capacity to plan and arrange fertility.

On the other hand, McCann does not let Sanger off the hook entirely. Sanger wanted "reliable" African American allies in the Harlem clinic and, later, in Planned Parenthood's Division of Negro Service, established in 1938. Worse, Sanger and the Sangerists engaged in what McCann calls "racial maternalism," that is, an ameliorative set of strategies in which "welfare feminists recast their gender solidarity with poor, ethnic women in a patron-client relationship in which the welfare feminists served as the models of successful womanhood. In this way, the programs' focus on the individual mother's behavior obscured the social basis of poverty. If a poor, ethnic mother strove hard enough to duplicate middle-class, old-stock standards of domestic and maternal excellence, then poverty would not thwart her efforts to raise 'good children' " (p. 127).

This criticism is at the core of McCann's assessment of Sangerism and reflects the problems at the core of the HistCon method. HistCon scholars are both respectful of the text and keenly aware of themselves as historically situated observers, and both of these elements are clear in McCann's work. She notes, for example, that she, unlike an earlier generation of scholars, is writing in an era of rollback of feminist gains and is driven therefore to understand the "costs of partial success." But can she do this only within the boundaries of the texts themselves? What this reader wants—and what scholarship in this area needs—is some old-fashioned, narrative political science, where the author takes on the stance of omniscience in order to draw some hard and perhaps controversial conclusions.

So Sanger was not personally racially biased but did not give her African American allies a free hand? So she respected poor and minority women and endorsed a rhetoric of racial and class equality but seemed to think that birth control alone would do the job? How are we, from the distance of years, to assess Sanger and her movement? She was surely ahead of many of her peers in her values and, like every charismatic leader, was convinced that her movement was the opening wedge to a better world.

Curiously, McCann seems unaware of the work of Theda Skocpol, a strange omission in a book that seems otherwise enormously wide-ranging in its reading. In her recent *Protecting Soldiers and Mothers,* Skocpol, building on the work of Paula Baker, Katherine Sklar, and others, has firmly put the question of the possibilities and limits of "maternalist" politics on the scholarly agenda.

In the end, the key question is whether maternalist politics do more harm than good. Given that women on the whole command fewer social and political resources than men, are maternalist politics the only option open for women activists? Put concretely, are women (including African American women, immigrant women, and poor women) better off in 1995 than they would be if Margaret Sanger had never undertaken her crusade? How do we judge social movements such as these? What lessons do we need to learn, given that we, too, are almost certainly in the throes of making political decisions that will seem deeply flawed to later generations?

Carole McCann's *Birth Control Politics in the United States* cannot single-handedly answer these questions, and readers interested in such questions will continue to wish that she had shown little less enthusiasm for analyzing "discourse" and a little more enthusiasm for understanding "practice." But as a welcome and engaging addition to the field, this book merits attention because it provides the best case study to date about the birth control movement as a *women's* movement for social change.

University of California, Berkeley KRISTIN LUKER

Beyond Politics: Markets, Welfare, and the Failure of Bureaucracy. By William C. Mitchell and Randy T. Simmons. Boulder: Westview, 1994. 234p. $55.00 cloth, $19.95 paper.

Governments as diverse as those of China, Great Britain, Hungary, Argentina, and the United States have responded to an increasing recognition of the virtues of markets. The idea of government intervention or direct government control as a way to solve social and economic problems is on the defensive almost everywhere. On the other hand, there is resistance in places like Washington, DC, to the scaling back of public programs of various kinds.

How does one know where to stop in a reduction of government intervention? What is the proper balance between markets and government? We need a guide that would simultaneously recognize the virtues and failings of markets and the virtues and failings of government. *Beyond Politics* might be such a guide, because it is one of the few books to discuss market failure and government failure in a single volume. But unfortunately, this book is not an even-handed balancing of the strengths and weaknesses of these two fundamental alternatives. *Beyond Politics* is consistently against government and in favor of markets. The book begins with an attack on "welfare economists" as having an uncritical "faith in government activism" and the theory of market failure as having "dethroned" markets.

Now, some economists are surely guilty of such a one-sided view, but most economists that I know are simultaneously influenced by a respect for markets *and* an understanding that markets can sometimes fail and why they can fail. It is surely true that many academics, as well as many public officials and many citizens, are quick to see the limitations of markets but slow to see the counterpart possibilities of "government failure."

For people with an uncritical faith in politics and government, Mitchell and Simmons's book is a welcome antidote. Those looking for a framework for *comparing* government failure with market failure will still need to refer to Shepsle and Weingast's "Political Solutions to Market Problems" (*APSR*, 1984) or to Charles Wolf's *Markets or Governments* (2d ed., 1993). There is a need for a book that is aimed at a broader audience, as *Beyond Politics* is; but there is a need for a balanced treatment or one that at least strives to be neutral, which it is not.

The authors implicitly acknowledge that they have set up a straw man when they conclude their summary of the "Political Presuppositions of the Idealized State" with the following: "Although it is doubtful that many economists who advocate government intervention to reverse market failure are as optimistic or naive as the preceding summary suggests, the summary does reflect the view animating discussions promoting government to resolve market failures" (p. 35). Part 2, "In Dispraise of Politics," reviews some fundamentals of public choice, such as theories assuming that bureaucrats are selfish utility maximizers, showing how logrolling can make the participants worse off, and showing the inherent instability of majority rule. This section veers between hyperbole ("Collective choice is accordingly inherently coercive and irrational" [p. 63]) and wisdom ("No set of rules can guarantee the achievement of fair and efficient policies" [p. 80]). Most of the book is devoted to "Case Studies in the Anatomy of Public Failure." Chapters are devoted to public goods, producer-rigged markets, government exploitation, consumer protection, environmental policy, redistribution, and macroeconomic policy. I will make some specific comments on this latter area, which I know best. The chapter treats the political business cycle as an established fact, rather than as an interesting hypothesis for which the evidence is mixed. On the other hand, it is on target with its observation that much U.S. economic policy has discouraged savings and investment and that in spite of institutional reforms, the national government has been fiscally irresponsible. But it does not add up to full support for the claim that "the most important cause of modern economic fluctuations is not the economy but the polity" (p. 176).

The authors do have a refreshing capacity to turn their analytical tool on themselves (and us). It is only consistent with their style and theme that they point out that "even public choice analysts have a self-interest in complaining about large and active governments" (p. 178). And even though they both teach at public universities, their chapter on government exploitation has a section of "Profscam and Price Fixing at State Universities." Although this section is weakened by the book's general tendency to assume the worst about government, it is not without insight. The authors contend that "our age-old depiction of the university as a hallowed, disinterested searcher for truth and exemplar of altruistic virtues must be subjected to the same careful analysis we accord business firms, political parties, interest groups, and other organizations" (p. 123).

Beyond Politics concludes with a short section entitled "In Praise of Private Property, Profits, and Markets." This section outlines the virtues of markets as conveyors of important information and as efficient allocators of scarce resources; and it provides a list of suggested reforms, including privatization and a constitutional amendment to require a balanced federal budget.

There is a need for a public philosophy that would provide a framework for discussion of appropriate spheres for markets and government. One such philosophy might begin with a recognition of the desirable features of markets and also of the possibilities of market failure. But it would build on material such as is in this book to identify the risks of government failure, that is, the possibility that government intervention may well make things worse. Such a philosophy could provide a basis for communication and discourse and a framework for reasoned disagreement between people with diverse points of view about the goals of public life. The seeds of such a public philosophy are to be found in this book. But *Beyond Politics* is too one-sided in its views to provide a common ground.

University of North Carolina,
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WILLIAM R. KEECH

Judicial Power and American Character: Censoring Ourselves in an Anxious Age. By Robert F. Nagel. New York: Oxford University Press, 1994. 188p. $29.95.

Robert Nagel says in the introduction to his new book that it contains "much that will irritate the reader" (p. 6). He delivers on his promise. For those who hold a more liberal political and philosophical view than Nagel, many of the arguments in his book will reflexively raise hackles. For example, he speaks of the "profound delusion and arrogance" in the jurisprudence of Justice William Brennan, dismisses the Supreme Court's opinion in *Wallace* v. *Jaffree* (striking down an Alabama "moment of silence" law for violating the establishment clause) as a form of "censorship" and "mind control," argues that the Court in *Eisenstadt* v. *Baird* took a "strikingly condemning stance" when it characterized as "irrational" those prohibitions on the distribution of contraceptives that distinguished between married and unmarried persons, suggests that it was the Court (rather than the laws it struck down) that was irrational in the flag-burning cases of *Texas* v. *Johnson* and *U.S.* v. *Eichmann*, and concludes generally that judicial review—exercised by "morally imperious judges" who are "self-deluding and intolerant"—should give way to self-confident, majoritarian politics exercised through our democratic institutions (pp. 7, 111–12, 128–29, 131–32, 134).

But Nagel does not reserve his criticism for liberals. Indeed, the book explicitly claims to cross conventional political and ideological lines. Thus, Nagel criticizes Robert Bork, as well as Laurence Tribe and Ronald Dworkin (saying, e.g., that Bork's iconoclasm "sometimes verges on being frivolous or even nutty") (p. 34). He sees the same "delusion and arrogance" of Justice Brennan in the common law incrementalism of the second Justice Harlan. He questions the "restraintism" of James Thayer, as well as the "censorship" of political correctness. He explicitly includes himself in what he criticizes (p. 6) and makes it clear that his goal is to understand judicial power better, to demythologize it, rather than to change the process or to offer a theory of judicial review.

Nagel argues that both activists and restraintists often criticize others for doing what they themselves do. Thus, those who reject politically incorrect statements as a "mockery of freedom of expression" are themselves censoring expression. Judges who strike down laws because they reflect an "unfocused" governmental interest often do so based on an equally "unfocused" judicial interest. Nagel points to Justice Brennan's opinions in obscenity and flag-burning cases and argues that Brennan's "boundless justifications" pose just as much of a hazard as the original laws did (p. 97). Likewise, both sides in the 1987 confirmation battle of Robert Bork portrayed the fight as one between "law" and "politics," but each claimed that their *opponents* were the ones stooping to politics. Thus, Bork's supporters argued that he stood as a proponent of principled legality (law as discipline) and claimed that his opponents, eager to advance the agenda of the Democratic Left, represented politics as desire when they rejected his nomination. Bork's critics claimed just the opposite. They argued that Bork, a constitutional radical who stood outside the mainstream, represented the threat of politics, while the Senate exercised restraint in rejecting him (p. 30). Such paradoxes permeate our legal discourse.

Nagel starts his book by claiming that the concept of the "landmark case"—one in which the Court heroically intervenes to assert constitutional principle—is mostly an illusion (and a dangerous one, at that). He notes that the Court usually reflects political currents, rather than leading them (p. 1). Thus, he sees the Court as a barometer of political culture and argues that contemporary judicial review is a reflection of moral decline and cultural self-doubt. It is this pervading self-doubt, he argues, that reinforces "the sense that people are unworthy (and in need of improvement)" and that has led to our willingness to accept the censorship of judicial opinions (p. 142). The regulation of sexist speech is one example, as was the Court's opinion in *Wallace* v. *Jaffree*. But censorship can also be achieved indirectly. Name calling, for example serves many of the same functions as outright censorship by frightening people into silence (p. 123). Nagel cites the political correctness movement as one that employs such a tactic but adds that invective routinely infects Court opinions where "the name-calling and exaggeration that mark the lower depths of political debate are simply given a more acceptable, authoritative form" (p. 129).

Of course, invective has also infected the Supreme Court confirmation process, and Nagel devotes a chapter to the recent confirmation battle of Clarence Thomas. Nagel argues that the Thomas hearings provided symbolic confirmation that American culture is in trouble (p. 10). Here, again, the hearings posed abundant paradoxes. He argues that "a central principle of the Supreme Court's free speech decisions is that offensive communications are valuable," but Thomas's alleged verbal harassment against Anita Hill put us in the awkward position of reconciling this solicitude for free expression with the special treatment that we reserve for sexually offensive speech in the workplace (pp. 14–15). Gnawing doubts about racial preferences served as a subtext to the hearings, as did the spectacle of politicians on the Senate Judiciary Committee presuming to apply ethical standards (p. 22). The result was an odd combination of political cynicism and personal self-righteousness (p. 24). Ironically, Nagel notes that Hill's allegations prompted a sexist response from those trying to help her. "The predominantly male Congress quickly rode to the rescue," Nagel writes, "gallantly expanding federal statutory protections," thereby trading on old "stereōtypes of female vulnerability and male strength" (p. 18).

In the end, Nagel may not cross conventional political and philosophical lines as much as he claims to. Instead, his arguments reassert that law is politics and that theories to constrain judicial power (whether those of Thayer, Bork, Dworkin, or Tribe) ultimately do not cabin judicial discretion and may actually promote it by giving judges the opportunity to use the exercise of "principle" to mask power. Nagel fears judicial power and favors majoritarianism. Those who hope for renewal, he concludes, should look for it in our democratic institutions, rather than courts. In fact, the basic argument of *Judicial Power and American Character* is similar to that of Nagel's previous book, *Constitutional Cultures* (1989), in which he argued that the routinization of judicial power is one of the main enemies of the constitutional order.

I do not agree with all of Nagel's conclusions, but there is much in his thought-provoking and well-written new book that is on the mark. At times, he irritates precisely because his observations reveal more about ourselves and our judicial theories than we care to admit. For those interested in the ongoing debate over the role of judicial power in our system of government, this is a valuable book.

University of Georgia JOHN ANTHONY MALTESE

The Myth of the Modern Presidency. By David K. Nichols. University Park: Pennsylvania State University Press, 1994. 181p. $32.50 cloth, $13.95 paper.

David Nichols likes a good argument. In this interesting and lively defense of the strong presidency, he takes on all those (most scholars, he says) who trace the office to Franklin Roosevelt or Woodrow Wilson or any of their illustrious predecessors. We are not living in their shadows, according to him but in that of the framers; for in its essentials, the presidency today is what it was in 1787. If it is imperial, it is because the Constitution made it that way and scholars fashion a myth if they believe otherwise. Indeed, the first modern president, we are told, was George Washington.

To make his case, Nichols goes to the Constitution's provisions and to what the founders said about them, sometimes dressing them in the terms of modern discourse. For example, the president's duty to recommend measures to Congress is expressed as making him an agenda setter, and Gouverneur Morris's remark in the constitutional convention that the president would guard the people against legislative tyranny serves to make him a popular leader. But as Nichols observes, not implausibly, the founders may not have understood completely the potential of what they created.

Nichols acknowledges that important changes have taken place in the presidency in this century, especially, it appears, since the time of Franklin Roosevelt. There have been great increases in the size of the presidential staff, in the exercise of presidential discretion, and in the president's engagement with the Congress; and thanks to electronic media, the president has become a greatly expanded presence in the lives of the American people. The myth of the modern presidency is true to some extent. But, he contends, such developments have not effected a transformation of the constitutional office; at least, the constitution is largely responsible for the way the office has developed. This raises an interesting question: When do quantitative changes become qualitative ones? Was Alexander Hamilton writing anonymously for the newspapers in support of Washington's neutrality proclamation doing essentially what Bill Clinton does in appearing as televised "town meetings"?

Nichols concedes that recent presidents may be doing too much, but the problem here, he says, is that the character of government and society have changed. Perhaps he has in mind the tendency on the part of all members of government, including Supreme Court justices (in interviews and in the way they write their opinions), seek to win support for themselves and their positions by "going public." I take Nichols's concession to mean that all of government has become imperial. It may be true, as he insists, that the balance among the branches has not been affected by such changes, but that is another matter.

Nichols knows his subject and has thought a lot about it, and he throws off provocative observations as he moves through it. For example, he observes that parliamentary government, far from being more advanced than one based on separated powers, is an arrested stage in democratic development. I do think that he has overstated his case and thereby weakened it. It is not enough that the founders had devised a strong presidency; they must have fashioned a "powerful" one (p. 173). Nor is it enough that Hamilton (among others) believed the president could engage in war once another country started it against the United States; he must allow the president (if I understand Nichols correctly) to initiate virtually any military action, including limited wars on the scale of Korea and Iraq. Is this the presidency whose leading craftsmen in the convention feared would be overmatched by a turbulent Congress, which James Wilson said would not be a formidable office, and which Gouverneur Morris believed would be infirm unless combined with the judiciary to resist the encroaching and oppressive legislation of the legislative branch? And is this the Hamilton who could say no more than that without the consent of Congress, the president, to protect American commerce against France in 1799, could use naval ships as convoys "with authority to repel force by force (but not to capture), and to repress hostilities within our waters" (Hamilton's *Papers*, XXI. 461–62)?

In view of his overall argument, it is surprising that Nichols should throw doubt on two major constitutional props of the strong presidency. He thinks it is an open question whether the president may solely remove those executive officials who appointments require the consent of the Senate. Madison, however, maintained in debate in Congress in 1789 not only that the president had the power (Congress agreed with him) but that it was derived from the vesting clause of Article II ("The executive power shall be vested in a president"), which gave the president authority (in this case control over the executive branch) not limited to the constitution's enumerated powers. And Hamilton cited this debate in 1793, in his defense of Washington's issuance of a neutrality proclamation when war broke out in Europe, to support his contention (accepted by nearly all strong presidentialists) that the Article II's vesting clause included general control over military and foreign affairs (limited by Congress's power to declare war and the Senate's power to approve treaties). Further, Nichols doubts that the president's duty to see to the faithful execution of the laws accords him any extraordinary powers. However, in the 1789 debate, Madison specified this clause as an alternate ground for the president's power to remove; and in his 1793 defense, Hamilton cited it as an alternate ground for the issuance of Washington's proclamation.

Boston College ROBERT SCIGLIANO

Regulation, Organizations, and Politics: Motor Freight Policy and the Interstate Commerce Commission. By Lawrence S. Rothenberg. Ann Arbor: University of Michigan Press, 1994. 315p. $39.50.

The Interstate Commerce Commission (ICC) was the first independent federal regulatory commission. Although now facing possible extinction, for a century, the ICC was the model for most of the economic regulatory bodies that were created by both state and federal government. The ICC also has been important to social scientists, being the subject of countless scholarly books and articles. The consensus scholarly view is that the ICC illustrates an unfortunate pathology of regulation—capture by special interests (in this case, the trucking industry).

Because published research on the ICC is so extensive and includes some of the best and most respected scholarship on economic regulation, young scholars might well think twice about earning their spurs by undertaking still more research on the agency. Nevertheless, Lawrence Rothenberg has managed to say something fresh, new, and important. His book is a must for any scholar who wants to keep current about research on regulation.

Rothenberg's book tests the neoinstitutionalist rational actor theory of the creation and implementation of public policies, but it tests this approach only in the broadest, most eclectic sense. Rothenberg masterfully integrates the insights of those who focus on Congress, the presidency, external organized interests, the courts, the bureaucracy, and administrative procedures to construct a remarkably integrated, balanced model of ICC decision making. Rothenberg's model focuses on the organized interests that were most significantly affected by transportation regulation but takes into account the organizational structure through which groups gain influence.

After chapters setting forth his basic conceptual model and the history of the agency (focusing on how the ICC obtained and carried out the authority to regulate trucking), the core of the book analyzes the most important regulatory decisions by the ICC: entry, mergers, prices, and eventually the transition to deregulation. The chapters on entry and mergers are the most important contributions to research on the ICC in several years.

Entry is the domain of ICC activity where capture was most prominent, yet Rothenberg demonstrates that a complete understanding of the ICC on entry cases requires understanding the institutional setting in which interest groups influenced the agency. The heart of this chapter is probit analyses of agency decisions and individual commissioner votes on 6,307 entry proposals between 1947 and 1980. The equations include regressors to measure congressional preferences, presidential influence, judicial oversight, interest group lobbying, and organizational and procedural aspects of the case. Statistical significance and quantitative importance is distributed generously among all theories of agency behavior! The clear, convincing conclusion is that the traditional view of capture by trucking firms and their union is a solid but partial explanation of ICC decisions; indicators of the institutional features of the agency also explain much of its behavior.

The chapter on mergers, if somewhat less successful, continues the same basic theme. Mergers, more than entry decisions, can divide the regulated industry, and so represent more of a challenge for testing theories of interest group influence. Rothenberg estimates essentially the same equation for 1,347 merger proposals as he did for entry decisions. The regressors are much less richly endowed with statistical significance, and coefficients are more likely to be sensitive to specification. The most important influence on merger decisions is case-specific lobbying by interest groups. Rothenberg's conclusion here is very interesting. Whereas interests could influence outcomes by lobbying specific cases, usually they did not choose to do so. Instead, they apparently focused their limited resources on other political activities with, presumably, a higher payoff. Thus, concludes Rothenberg, to understand how and whether a policy is captured, one has to study not just opportunity (the focus of the interest group theory) but the internal decisions of interest groups about where to apply leverage. Frequently, interests eschewed the possibility of influence despite the opportunity for it.

The stories about prices and deregulation are less of a departure from established wisdom, most likely because both have been the focus of much previous research on the ICC. Most pricing activity took place in ICC-sanctioned regional cartels; the ICC stood by to arbitrate the infrequent disputes among cartel members. Rothenberg concludes that the ICC was not a neutral arbitrator, systematically resolving disputes in favor of the less-than-truckload carriers and the Teamsters, at the expense of other parts of the trucking industry and other transportation modes.

In dealing with deregulation, Rothenberg argues that the old regime became too costly, bankrupting the railroads and inflicting enough harm on enough shippers that reform of surface transportation regulation became politically salient to them. The first response to rising political demands for reform was from the presidency, which began nominating reform-minded commissioners and advocating more competition. Division among interests prevented congressional defenders of regulation from blocking this presidential initiative. When Congress ratified deregulation in 1980, the deed had already been done internally.

The analysis in the book has but one notable analytical problem, which it shares with almost all other studies of case decisions by regulatory agencies or the courts. Specifically, the number and nature of the cases considered is endogenous to the policy being advocated by the agency. A more lenient policy toward trucks will not necessarily lead to a greater proportion of decisions favorable to truckers because (1) truckers may alter their behavior to push the envelope of decisions still more in their favor and (2) opponents of truckers may simply reduce their propensity to contest trucking cases. How one deals with this problem remains a very open question; endogenizing case characteristics is not easy.

The strength of this book is its exhaustive study of case decisions. As such, it nicely continues the important studies by Moe of the National Labor Relations Board (Control and Feedback in Economic Regulation," *APSR*, 1985) and by Magat, Krupnick and Harrington of rule-making in regulating water pollution (*Rules in the Making*, 1986), which also develops an eclectic rational-actor theory of the Environmental Protection Agency and tests it using case decisions in the 1970s. Case decisions, and linkages between cases and congressional oversight activities, remain the most underexploited empirical resource in studying regulatory agencies. Rothenberg's success in using case data to shed new light on the nation's most studied regulatory agency should inspire others to adopt this research strategy.

Stanford University ROGER G. NOLL

The Nemesis of Reform: The Republican Party During the New Deal. By Clyde P. Weed. New York: Columbia University Press, 1994. 293p. $37.50.

This is a spare and briskly argued examination of the behavior of the Republican party during the New Deal of 1933–38. Clyde Weed's principal analytical point is that the Republican response to the New Deal—a combination of bitter disarray and sulky partisanship—challenges the core assumptions of realignment theory (as advanced by Anthony Downs, James Sundquist, and others). Instead of following the majority party in pursuit of partisan equilibrium, the Republicans sit in futile opposition until the late 1930s when (for largely serendipitous reasons) the Democrats and the larger debate drift back to more familiar terrain. Citing both anecdotal evidence and patterns of congressional voting, Weed traces GOP irrationality to four factors: the dizzying challenge and change of the early Depression, the fundamental and irreconcilable split in party ranks between northeastern party regulars and western insurgents, the divergent demands of "interest groups" and voters, and the short-sighted ignorance of party officials who (in the absence of dependable polling) consistently misread both the public mood and their own appeal.

The narrative scheme is fairly straightforward. Weed begins by tracing the political economy of the Republican 1920s, underscoring the practical and political fragility of "associationalism" after 1929 and (accordingly) the willingness of western insurgents and business to bolt to FDR in 1932. This disarray continues through the 1934 elections, as western Republicans support the administration's agricultural policies, and eastern party regulars wait for the New Deal to stumble. While the Republicans seemingly have their problems sorted out for them by the left turn of Second New Deal in 1935, Weed suggests that the party is painfully slow to assume leadership of increasingly vocal anti–New Deal forces. This problem is set in starkly material terms: contemporary tallies of campaign contributions show substantial business support for Roosevelt in 1932 and an even more substantial rush back to the GOP in 1936. If economic interests had hastened Roosevelt's election in 1932, why could they not defeat him in 1936? Weed's answer is twofold: (1) Republicans read the editorials and campaign ledgers and casually overestimate the strength of the conservative backlash; and (2) in part because they take this groundswell of conservatism for granted, party leaders devote their attention to the now gaping rift between the party's western and northeastern factions. Accordingly, the 1936 nomination goes to Alfred Landon of Kansas and the 1936 platform (often tortuously) strives to appeal to both western progressives and eastern conservatives, a combination that proves disastrous.

In the final chapters, Weed retells his story and deepens his analysis by focusing on the voting patterns of congressional Republicans. The party's sectional problems emerge sharply in the disarray of the 73rd Congress as western insurgents consistently back the administration. The onset of the Second New Deal sharpens partisan lines (in part because many business interests have returned to the fold); but the 74th Congress (1935–36) sees the persistence of a sectional standoff. Only after 1937, amid the court-packing fiasco, the "Roosevelt recession," and the emergence of congressional (largely southern) conservatism within the Democratic party does the GOP achieve a semblance of unity and competitive stature. The sectional problem dissipates as some western progressives drift back to the GOP and others drift out of the party altogether.

Weed makes a solid case for the importance of Republican sectionalism and the perceptual scramble of 1933–36. But the connections between this story and our larger understanding of partisan behavior (esp. the ways in which Republican actions challenge the conventional logic of two-party politics) are less convincing. In this sense, Weed runs into four related problems.

First, he expects the Republicans to "realign" their program against New Deal reforms as early as 1933. Yet the New Deal of 1933–34 defied conventional left-right ideological distinctions (Republicans attacked the NRA as both the leading edge of socialism *and* an American variant of fascism). Furthermore, bipartisan consensus, recast around an acceptance of at least "commercial" Keynesianism, had substantially returned by 1938 (a point Weed makes without connecting it to his larger thesis). Second, Weed makes far to much of "program" in a notoriously nonprogrammatic, constituency-service party system. If we judge the rationality of political parties by their biennial platforms (as Weed does in part), the GOP of the early 1930s is scarcely the only candidate for this sort of criticism.

Third, the evidence presented here suggests that as an investment-driven, constituency-service party, the GOP is perfectly rational. In one electoral cycle, it retrieves the lion's share of business and financial support; and it follows its western and eastern constituents (which, unfortunately for the national party, make increasingly contradictory demands) quite faithfully. Finally, Weed draws contradictory and often misleading conclusions from patterns of financial support for the two parties. Using Louise Overacker's contemporary tally of campaign contributions, Weed argues that business opportunistically supported the Democrats in 1932 but fled back to the GOP in 1936. Yet as Thomas Ferguson has tirelessly argued (most recently in a 1991 exchange with Michael Webber in *Sociological Perspectives*), Overacker's statistics both dramatically understate business support for the Democrats and obscure the industrial and sectoral character of each party's support. Business views of the post–NRA New Deal reflected sharp, tactical, competitive, and structural divisions within the business community. Protectionists, open-shop advocates, southerners, and smaller competitors flocked to the GOP; but internationalists, capital-intensive and technological industries, and some chronically disorganized labor-intensive industries proved willing (if not eager) to accommodate the New Deal.

These patterns raise a host of questions—the role of international issues in the politics of the 1930s, the depth and sincerity of the Democrats' "class" politics, and the relative importance of voter preference and business investment in partisan realigment—that Weed leaves unanswered.

University of Iowa COLIN GORDON

Bureaucratic Dynamics: The Role of Bureaucracy in a Democracy. By B. Dan Wood and Richard W. Waterman. Boulder: Westview, 1994. 190p. $59.95 cloth, $19.95 paper.

Using an impressive array of sophisticated methodological techniques, Wood and Waterman focus on the outputs of eight federal regulatory agencies and prove that the journalists were correct about what happened during the 1980s; that is, Congress and the president often competed with each other to control the bureaucracy. Furthermore, by applying the tools of political control that the two main branches of American government share (esp. political appointments and budgetary resources), one or the other branch often succeeded in getting bureaucracy to change its actions. In addition, the authors found that the courts, the media, interest groups, dramatic events, and citizens also affect bureaucracy. Finally, the authors' case studies of the EPA's clean air enforcement actions and the Nuclear Regulatory Commission's safety enforcement actions indicate that rather than being passive objects controlled by other actors, bureaucracies also "have power in their own right and sometimes use that power to alter outcomes in their relations with other actors" (p. 126).

The book, which brings together the authors' previously published writings on this subject, reflects scholars' emerging consensus that bureaucracy is highly adaptive to changing political situations. Although the main findings may seem less than spectacular to those who followed public affairs during the period studied, Wood and Waterman display considerable initiative in uncovering—through Freedom of Information Act requests—time-series data that are more detailed than those bureaucracies publish. Furthermore, the authors analyze the data skillfully and provide us with a clear, if impressionistic, portrait of the political responsiveness of federal regulatory agencies during the 1980s. (The assertion that their results "are generalizable to the entire federal bureaucracy" [p. 32] must, however, be interpreted as hyperbole, unless the results are interpreted so broadly as to make them virtually irrefutable.) An additional contribution of their empirical work is that by emphasizing the roles of the president and the Congress as a whole in controlling bureaucracy, they cast further doubt on the usefulness of the "iron-triangle," "subsystem," and "capture" metaphors of bureaucratic analysis. Finally, their recommendation that the kinds of data they uncovered be regularly published as a means of keeping both bureaucrats and politicians responsible seems generally sensible. The main drawback of their "policy-monitoring" idea is that it would encourage the trend toward defining bureaucratic work in purely quantitative terms (whether or not such indicators are appropriate), which may skew behavior in ways that are counterproductive for performance of the bureaucracy's mission.

In addition, the book is suffused with the authors' theoretical perspective, which comes from organizational economics—the principal-agent model. Though it does not seem relevant to their empirical findings, it is crucial to an overall evaluation of the book. Joseph E. Stiglitz states, "The principal-agent literature is concerned with how one individual (say an employer) can design a compensation system (a contract) which motivates another individual, his agent (say the employee), to act in the principal's interest" (*The New Palgrave*, 1987, p. 966). The so-called "principal-agent problem" results from the inability of the principal to observe all of the agent's actions (*information asymmetry*). The necessity to delegate responsibility to agents will virtually always allow the agent to *shirk*. The shirking-information problem exposes principals to deceptive behavior by agents: *adverse selection* at the precontract stage and *moral hazard* at the postcontract stage (in the jargon of the insurance business).

Like most economists, the political scientist authors of the book under review pay remarkably little attention to their assumptions, saying only, without attempt at justification, that during the 1980s, principal-agent models "soon came to be viewed as applicable to relations between public managers, who are often elected, and their employees in the bureaucracy, who are often tenured" (p. 22). But *why* did some people see the model as applicable? Chief among the problems of applying these notions to the control of bureaucracy is identifying the principal. (The need to do this, e.g., under *total quality management*, to hold a seminar to find out who is the public bureaucracy's "customer," is a dead giveaway that one is dealing with a metaphor, rather than a theory). Anyone having more than a nodding acquaintance with American government knows that the competition between the Congress and the president is a main problem and that (as these authors have demonstrated) attempting to control bureaucracy is a main venue in which the problem is played out. Who, then, is the principal of the contract in question? The clear answer is that bureaucracy has two main principals and several lesser ones, any of which can make at least a limited claim to legitimacy. These are the central and obvious features of American bureaucracy's political predicament.

Under these circumstances, determining when bureaucracy is shirking is usually impossible, because it is likely to be acting as one of the principals has asked. The notion of competing, *multiple principals*, endorsed by Moe ("The New Economics of Organization," *APSR* 1984, pp. 768–69) and the present authors (pp. 147–49), seems nonsensical from the point of view of the original model. Thus, it is reasonable to conclude that the existence of competing principals invalidates a chief assumption of the principal-agent model and makes it inappropriate for application to the problem of political control of bureaucracy at the macro level. Using principal-agent jargon when the theoretical underpinnings for its use are absent does not contribute to political analysis.

Wood and Waterman make the normative judgment that the Congress is the more legitimate policy maker of the two main multiple principals and that the president's powers over bureaucracy should be reduced accordingly. Also, they believe that the actions of "political principals" and "bureaucratic agents" should be congruent with popular preferences, which become the ultimate principal. I wonder whether they adopted the principal-agent model because it was congruent with their prescription for a majoritarian democracy (which they propose uncritically) or whether they were attracted to the model by its rational elegance and the prescription flowed from the choice of model. In either event, elevating the principal-agent model to the status of a theory of democracy seems unwarranted.

University of Oklahoma LARRY B. HILL

Pitching the Presidency: How Presidents Depict the Office. By Paul Haskell Zernicke. Westport, CT: Praeger, 1994. 175p. $49.95.

As a title, *Pitching the Presidency* misleadingly suggests that the president's portrayal of the office is cynically manipulative, a claim at odds with the core ideas of the book. Focusing on the Johnson, Nixon, and Carter presidencies, *Pitching the Presidency* studies "how and why a president presented the office in a particular way, and how these depictions shaped public attitudes and discourse, and effected the presidency itself" (p. 5).

The book succeeds at showing *how* these three leaders portrayed themselves, their presidencies, and the presidency. Zernicke's account of the varying ways in which three presidents cast the office is both comprehensive and coherent.

Less cogent is the book's explanation of the reasons for these rhetorical constructions and their effect on the public. And the posited effects overstep the book's evidence. Misuse of the office, rather than rhetorical construction of it, probably accounts for most of the public's disenchantment with appeals to the presidency in the aftermath of Watergate.

The book charts the conflicting requirements that the public places on a president; examines the depictions of the presidency in campaigns, as well as in and out of office; charts portrayals in times of controversy and crisis; and argues that Johnson's and Nixon's appeals invited a backlash against the institution itself, paving the way for Carter's open, accessible, and insufficiently magisterial presidency. A quick pass over Reagan, Bush, and Clinton suggests to Zernicke that where Reagan emphasized majesty, Clinton is more Carter-like. The author closes with a call for a more realistic public and more candid presidents.

In his discussion of presidential responses to crisis, Zernicke would have benefitted from familiarity with Windt's *Presidents and Protesters* (1990). The work is marred, as well, by occasional indulgence in forms of psychobiography unrequired by its thesis and unsupported by its evidence—for example, "Presidents who have low self-esteem are especially prone to extreme interpretations of their responsibilities" and "Carter was not obsessive-compulsive about the office and the title 'President' " (p. 3).

In a similar vein, wholesale claims about the press either require qualification or more extensive documentation. "The press eventually condemned Nixon's policy as ignorant and isolated and questioned his authority to escalate the war," writes Zernicke (p. 43), footnoting a single article in the *New Yorker*. Also calling for evidence are such assertions as, "Johnson, Nixon, and Carter frequently opened, abbreviated, or closed a campaign speech with a discussion of the presidency" (p. 41).

In some instances, Zernicke's descriptions of campaign behavior are simply wrong, for example, his notion that in 1968, "conventional press interviews and stump speeches *were replaced with* a panel format where citizens directly asked Nixon questions" (p. 90, emphasis mine). And one is hard-pressed to confirm that "*many* of Reagan's anecdotes were film fantasies, not the real-life incidents he appeared to believe they were" (p. 147, emphasis mine).

The book invites the question, How does one know whether and when rhetoric is driven by conviction, conviction by rhetoric, or the two are divorced? Rather than holding that Nixon came to believe his own rhetoric (p. 87), why not conclude that his rhetoric reflected his beliefs? Similarly problematic is the statement, "There is nothing in [Carter's] pattern of depiction to suggest that he always believed his own rhetoric" (p. 50).

Nor are Zernicke's interpretations of the meaning of the rhetoric trouble-free. Why, for example, is "I must do what is right" interpreted as "a meek personal plea" (p. 2), rather than a powerful assertion of personal principle?

Despite its flaws, *Pitching the Presidency* is a useful catalogue of the ways in which three different presidents invited audiences to see them and the office.

University of Pennsylvania KATHLEEN HALL JAMIESON

COMPARATIVE POLITICS

Conflict and Cohesion in Western European Social Democratic Parties. Edited by David S. Bell and Eric Shaw. Pinter, New York: St. Martin's Press, 1995. 202p. $49.00.

This edited volume on the strategic and organizational problems of European social democratic parties in seven countries in the last 20 years is the latest addition to a small library of similar books that have appeared since the 1970s. Most of the contributors are accomplished scholars who have published monographs and articles on left parties in the past. In contrast to previous volumes, this book places special emphasis on the internal organizational dynamics of European social democracy in a time of strategic upheaval. Stephen Padgett describes how the German social democrats became an increasingly divided party as their hold on power crumbled in the late 1970s and especially since they entered opposition in the 1980s and 1990s. Martin Bull analyzes the transformation of the Italian communists from an organization governed by democratic centralism to a more open Democratic Party of the Left that permitted considerable leeway to internal opposition and thus, ironically, made it harder for the party leadership to overcome the programmatic shackles of the communist past defended by recalcitrant rank-and-file militants. Richard Gillespie retraces the steps of the Spanish socialists from the late 1970s in their journey from factionalism to central leadership by the Gonzalez-Guerra duo and back to a resurgence of a factionalism that is now more driven more by patronage than ideological conflict. David Arter, a long-standing academic companion of Swedish social democracy highlights how even the Swedish party has experienced considerable internal tensions in the 1980s and 1990s. His Scandinavian colleague Knut Heidar, in contrast, finds that the Norwegian Labour Party has been drained of internal conflict and that its organization has sunk into political irrelevance. David Bell and Byron Criddle give an update on their monograph of the French socialists and sketch the precipitous decline of the party into factionalism, driven by personal cabals in the intensifying contest for the Mitterrand succession since the late 1980s. Philip van Praag, Jr., follows the journey of the Dutch Labour Party from a moderate social democratic party to a party of the New Left in the 1970s and the first half of the 1980s and then back to a more centrist strategic line with new intraparty alignments in the 1980s and early 1990s. A similar swing is demonstrated by Eric Shaw for the British Labour Party, which reached its zenith of radicalism in the early 1980s and then moved back toward more moderate stances and a more centralized party organization in the later 1980s and 1990s.

The editors do not pretend to offer an encompassing theoretical framework into which these case studies can be inserted (p. ix), nor does their conclusion open up new theoretical or comparative vistas. Nevertheless, a few useful observations and remarks can be gleaned from their comparative discussion, such as that the common wisdom asserting the counterproductivity of factionalism and of radical intraparty currents for the electoral performance of social democracy cannot be generalized. Overall, the book offers a handy summary of important events and processes in the history of social democratic parties in the seven countries it covers. To employ this book as an input into theory building and more rigorous comparative analysis, it would have been preferable had the editors managed to get the authors to address analytically similar questions in each case study. It would have been also desirable to display some basic data on the development of the parties, such as their annual membership change, voter/member ratios, and income; the nomination rules of parliamentary candidates; the frequency, timing, and procedures at national party conventions; and the periods of their participation in government, together with the portfolios the parties held. Nevertheless, the book provides a convenient compendium on recent political developments in European social democratic parties that may serve graduate students and scholars as an initial sourcebook on the way to more structured and theoretically demanding comparative investigations.

Duke University HERBERT KITSCHELT

The Problem of Democracy in Cuba: Between Vision and Reality. By Carollee Bengelsdorf. New York: Oxford University Press, 1994. 229p. $45.00 cloth, $16.95 paper.

The Problem of Democracy in Cuba is a book with two distinct parts. It begins with three chapters exploring the roots of the divorce between socialism and democracy, first in the writings of Marx and Engels, second in those of Lenin, and finally in the socialist practice of the Soviet Union. The argument weaving these chapters together is that the texts of Marx, Engels, and Lenin, while manifesting emancipatory democratic threads, exhibit deep contradictions, confusing ambiguities, damaging oversimplifications, and critical silences in their treatments of the relationship between socialism and democracy. Carollee Bengelsdorf argues that these textual weaknesses bear much responsibility for the undemocratic character of Marxist-Leninist practice in the Soviet Union, which in turn, had deleterious antidemocratic impacts on the practice of socialism elsewhere.

While I found Bengelsdorf's contention that much of the distortion of democracy under socialism can be traced to canonical problems strained, these opening chapters provide an engaging and impressive historical survey of the connection between democracy and socialism from Lenin through Stalin. This argument, however, assumes less prominence (though it does not disappear) in the second portion of Bengelsdorf's book, in which she analyzes democracy in the context of socialist Cuba.

Here she focuses on a succession of crisis periods when the issue of democracy was explicitly raised and problematized in Cuba and when attempts were made to overcome its uneasy coexistence with socialism. Bengelsdorf argues, however, that at each historical juncture, Cuba failed to escape the manacles of its socialist textual inheritance: "If Cubans addressed the theoretical legacy," she writes, "they did not redress it" (p. 9).

I welcome Bengelsdorf's serious effort to broaden our understanding of Cuban socialism, which, as she notes, has never been a mere tropical xerox of its European relatives—a fact to which its post-1990 survival, if nothing else, attests. I also think her choice of democracy as a lens through which to attempt this is worthwhile. Yet while it is inarguably true that scholars have not grasped the workings of actually existing socialist societies with anywhere near the sophistication that matches their analyses of actually existing capitalist ones, there are two particular streams of scholarship on socialism that, had Bengelsdorf taken note of them, would have considerably strengthened her book. The first, typified by authors such as János Kornai (*The Socialist System*, 1992), has uncovered the systemic roots of the underproduction (shortage of supply relative to demand) that permeates all levels of the system. Bengelsdorf's failure to take this fundamental dynamic as a starting point from which to develop her critique of representative government, or *Popular Power*, dulls her analysis. By both underplaying the importance of shortage for the functioning of Popular Power and by interpreting many shortage-induced difficulties with Popular Power as evidence of its undemocratic character, Bengelsdorf constrains her ability to uncover and to understand more fundamental democratic limitations of representative government in Cuba.

The second stream of scholarship that Bengelsdorf largely ignores, exemplified by György Konrád and Ivan Szelényi (*Intellectuals on the Road to Class Power*, 1979) and Michael Kennedy (*Professionals, Power, and Solidarity in Poland*, 1991) fruitfully focus on class dynamics and class politics under socialism. In marked contrast to her favorable reference to class analyses of prerevolutionary Cuba, Bengelsdorf is silent on the subject of class in post-1959 Cuba, in part because she attributes the shortcomings of classical Marxist writing on the transition to socialism to their class reductionism. This point is an important one, yet it does not justify the absence of any discussion of class and its relation to democracy under socialism. Indeed, had Bengelsdorf considered class in her treatment of democracy in Cuba, it would likely have led her beyond her reliance on paternalism, bureaucracy, generational difference, and even psychology (e.g., Castro's "natural instincts to hold on tightly to the reins" and "the Revolution's seemingly unquenchable need to . . . contain and control" [pp. 145, 149] as explanations for the undemocratic features of Cuban socialism and toward a deeper, more nuanced analysis of this topic.

As a consequence of these oversights, Bengelsdorf is able to proceed with her discussion of Cuban democracy in a fashion that too much isolates it both theoretically and empirically from its political, economic, and social context. The result is the common tendency, for example, to simplify the topic of democracy by reducing it to a discussion of state processes and structures and by trying to understand it as a series of unrelated dichotomies like decentralization versus centralization and dependence versus autonomy. Nonetheless, Bengelsdorf is to be credited for tackling such politically and intellectually important—albeit unpopular and controversial—topics as Cuba, socialism, and democracy. I always learn more from books like hers that attempt a great deal and fall somewhat short than from well-executed studies of relatively inconsequential matters.

University of Oregon LINDA FULLER

Labyrinths of Prosperity: Economic Follies, Democratic Remedies. By Reuven Brenner. Ann Arbor: University of Michigan Press, 1994. 312p. $45.00 cloth, $17.95 paper.

This book is full of so much common sense that the academy will surely ignore it. In it, Brenner attacks many common orthodoxies about how societies should cultivate a prosperous economy. These attacks focus on macroeconomic theory and practice, the design of democratic institutions, and central planning. The central thrust of the book is that all extant theories focusing on economic performance ignore basic fundamental tenets of economic activity: people must take risks and innovate if an economy is to prosper.

Brenner first takes on the misuse of poorly measured aggregate economic indicators. Recently, economists have begun to realize that measures of national income and other aggregate economic statistics are poorly measured and must be refined over time. An initial estimate of real disposable income has been estimated to have a standard deviation of 2.2%. This type of error makes useless the types of macroeconomic policies called for by economists who favor discretionary policy. Good data are needed immediately for discretionary policy. But far worse, the fixation on these aggregates distracts attention from what really makes economies work—institutions at the micro level that provide individuals with incentives to take risks, innovate, and invest. Brenner goes further, arguing that these statistics have nothing to say about whether a society is prosperous or not, and so these statistics should not be measured.

Brenner uses rich historical accounts to explain why societies either economically prospered or stagnated in various historical eras. One particularly good example is the Dutch case. The typical economist's explanation for Dutch growth in the seventeenth and eighteenth centuries is that the Dutch had a high savings rate. Nonsense, writes Brenner. The Dutch were successful because political institutions supported property rights that rewarded economic innovation. Venice provides an example of decline. Its economic success led to efforts to expand the powers and size of government. More and more attention was given to regulating economic activity, instead of taking part in it.

Brenner is more interested in providing a general approach to understanding how economic institutions generate innovative economic activity than in offering a menu of economic institutions to policymakers. In fact, there are many economic institutions that may work as long as they give individuals the incentive to take risks and innovate. He is very much against general arguments that say, for example, that government deficits cause economic decline. Whether a deficit is a problem depends on what the deficits are used for, he concludes.

Governments lose sight of what makes an economy work when the economy is performing at its best. The Keynesian neoclassical synthesis and contemporary macroeconomic practice (together with government ineffectiveness) obfuscate what makes economies operate effectively. Democratic and bureaucratic institutions are all too often not designed to remedy persistent error in the ways policies are made. Rather, political institutions are usually designed in a way that errors become compounded over time. Not only does this lead to poor economic performance, but when governmental policies interfere with markets, governments effectively replace ambition and trust in the governed with envy, resentment, and passivity. He uses a variety of examples from Russia to illustrate the effect that poor governmental performance can have on ambition and trust.

Up to this point, Brenner's argument is sound and provides a perceptive core with which it is difficult to argue. One might quibble with the idea that we should completely abolish collecting data on national accounts and other economic aggregates. (Brenner does see a place for price and monetary data in managing monetary policy.) But Brenner is correct in identifying the tendency for policy analysts to lose sight of how an economy actually operates. In fact, Brenner's arguments appear to fit squarely into the new institutional economics school and could be made more persuasive by drawing on many insights by Williamson, Demsetz, Alchian, and others. However, he does not recognize this, as far as I can tell.

In fact, he ends the book with a normative prescription that could use a healthy dose of new institutional skepticism. He argues that initiative and referendum, as practiced in Switzerland, could provide mechanisms that correct error that typical representative democracies allow to persist. Initiative and referendum can do so by, (to use social choice terminology) dividing the question. Voting for a candidate is the same as voting for a bundle of policies, and referenda provide a separation between the powers to tax and spend because each issue can be considered separately.

In choosing a single, best political institution, Brunner succumbs to what Demsetz (*Journal of Law and Economics,* 1969) calls the *nirvana approach,* as opposed to the *comparative institutions* approach. The nirvana approach searches for an institution that will be most efficient within any given context. The comparative institution approach is not designed to find a single, best institutional solution. That Brunner makes this mistake is interesting, given the fact that in comparing economic (as opposed to political) institutions, he is very sensitive to context.

It is unfortunate that after spending most of the book with a very compelling argument, Brenner decides to spend such a small amount of time and effort on an institutional solution. On the surface, the argument in favor of initiative and referendum appears a plausible solution. But he refers nowhere to a rich literature on social choice. His use of referenda is not unlike Riker's use of elections as mechanisms for throwing out bad politicians (*Liberalism against Populism,* 1982); but he fails to make this connection.

From a social choice perspective, it turns out that issues are much more difficult to divide than Brenner admits. And referenda can often become confidence votes for a government, forcing the issue space, again, to have high dimensionality. If we looked to California as a representative example of how initiative and referendum works, we would come away with a different perspective. If the public does not have vast amounts of information, powerful interest groups might be the decisive influence in any one vote, because these groups will be able to control information flows.

I would like to emphasize that its failing to find an institutional fix to the problem of democratic governance of an economy is little reason to ignore this book. This question of governance eludes everyone. Anyone who thinks about economic performance as a function of aggregate savings, disposable income, and all the rest of the economic aggregates has a lot to learn from this book.

Indiana University JOHN T. WILLIAMS

The Politics of Human Rights in Argentina: Protest, Change, and Democratization. By Alison Brysk. Stanford: Stanford University Press, 1994. 291p. $45.00.

All good books grapple with an intriguing puzzle. In *The Politics of Human Rights in Argentina,* Alison Brysk asks, How is it that a group of citizens who were politically weak, inexperienced, marginalized, and lacking in material and organizational resources were able effectively to resist a brutal regime and go on to play a key role in the transition to democracy?

The question is critical to those who can recount the period of authoritarian rule in the Southern Cone, when, at least initially, the horrors of the Pinochet regime in Chile seemed to capture much more attention than did the atrocities committed at the hands of the Argentine junta. This was so because the more internationally connected Chilean political parties could launch resistance efforts from abroad. Moreover, the Chilean realities were brought home to U.S. citizens once it was revealed how their own government had conspired to undermine the Allende administration and pave the way for military takeover. By contrast, Argentina was more of an unknown quantity—until, that is, the Mothers of the Plaza del Mayo emerged out of nowhere suddenly to focus public outrage against the dictatorship.

How were they and other human rights activists able to do this? The author provides a fascinating and theoretically informed answer by raising to prominence the theory and practice of symbolic politics. Rather than measuring impact in the alteration of the balance of power, she assesses the power of persuasion through the transformation of language, norms, culture, and ultimately institutions. In examining the human rights movement that developed in Argentina in the late 1970s, she argues that "the concept of human rights was established as a permanent reference for political discourse" there (p. 15). In what must be the strictest test of that hypothesis, the author shows quite convincingly how the repressive military regime itself felt obliged to adopt the language of human rights in defending its own actions.

The human rights movement compensated for its frailties by discovering the force of expression, communication, and symbol: "Truth and values can create their own kind of power" (p. 171). Groups wage their struggles over the meaning and interpretation of prior events, as well as the legitimacy and morality of political actions. On this battle field, the human rights movement often has the advantage, because the messengers (mothers and grandmothers of the disappeared) are credible and their message (the promotion of human rights) is nonpartisan.

Having once discovered the benefits of symbolic politics, the human rights movement in Argentina was able to transcend its own limitations by capturing the attention and support of organizations outside itself, building a coalition that ultimately helped to transform the political equation. Although vulnerable domestically, the movement was able to turn its weakness into strength internationally by leaping over nation-state walls to solicit the support of potential allies abroad. Using information as a tool of persuasion, it energized the will and influence of international nonprofit organizations, who, in turn, placed their own material resources at the movements' disposal. These transnational linkages empower movements and help to alter the behavior of regimes that suppress them.

Ironically, the human rights movement had its greatest success under the tyranny of military rule, not within the pluralistic environment of the new democracy. The military's intolerant, repressive and evasive tactics made the movement's morally dignified search for the truth, in the face of enormous risks, seem all the more impressive. But with the return of democracy, the movement's nonnegotiable claims and its lack of sufficient political flexibility were now liabilities in a system that placed a premium on compromise. It is to the credit of the author that while she is generally quite sympathetic to the human rights activists she studies, she is also quick to acknowledge the inherent limits to their influence.

Still, I think she tends to overstate the achievement of the human rights groups in having actually transformed Argentine political institutions. For example, she contends that if it were not for human rights pressures, the traditionally passive judiciary would have done little to redress military abuses. Actually, the Argentine judiciary has a rather strong tradition of autonomy from, and occasional defiance of, de facto regimes. It is more accurate to suggest there was a mutual affinity between the movement's objectives and the courts' modus operandi.

None of this, however, detracts from the important empirical and theoretical contributions made by this book. *The Politics of Human Rights in Argentina* brings to focus a movement that has not received its fair share of scholarly attention. It describes how a few grieving mothers moved from the political shadows to the spotlight of the international stage, and in doing so, discredited a repressive regime. But it accomplishes all this with theory as its guide, providing us with some innovative conceptual tools which, hopefully, will find application elsewhere.

University of California, Riverside David Pion-Berlin

Failure in British Government: The Politics of the Poll Tax. By David Butler, Andrew Adonis, and Tony Travers. Oxford: Oxford University Press, 1994. 432p. $39.95 cloth, $12.95 paper.

This gripping book recounts the British government's greatest domestic policy disasters of modern times, perhaps of the century: the devising, enactment, implementation, collapse, and replacement of the Community Charge, or poll tax (a standard-sum head tax in each local government district) as Britain's local government tax. It is a remarkable piece of investigative reportage, rather than an academic policy study. As such, it is a prime source for academic policy study and ideal for stimulating both policy scholars and their students into reflecting on the sheer difficulty and risk of making policy choices and the pitfalls of their implementation. Butler, Adonis, and Travers are a uniquely experienced British politics specialist, a *Financial Times* journalist, and one of the only "three or four people in England" who have long been held to understand the jungle of local government finance, respectively. (Travers regularly explained the poll tax's financial aspects on radio and television throughout the saga.)

The standard thumbnail sketch of this bizarre business is hostile to Margaret Thatcher, although by no means believed only by anti-Conservatives. It depicts a ruthless prime minister picking up the idea of the flat tax, ramming it through both Cabinet and a powerless House of Commons, on rigid "Thatcherite" ideological grounds. The gods observed this gross exercise in majoritarian dictatorship and so arranged to bring her down on this issue more than others. The poll tax's public unacceptability helped make her so unpopular that her parliamentary party saw her as a loser and withdrew enough support to force her departure. All this was fired up by major public demonstrations, including one big one in Trafalgar Square itself in London that turned very violent (albeit only at the hands of tiny groups of determined anarchists and Trotskyists, delighted at their chance). Thus, the poll tax debacle was often characterized by such expressions as "absolute power corrupting absolutely," "the harder they fall" (from the boxing world), and "autocracy tempered by riot," recollecting politics in post-Waterloo England.

These authors reveal a rather different story after interviewing every minister, civil servant, and policy adviser and every other contributor to the story whose name came up, except for Thatcher herself (who was not approached) and the one refusal (William Waldegrave, MP). (The latter was probably the most significant junior minister in the story and who, ironically, had climbed the greasy pole rather higher by the time of his refusal, to the Cabinet office of minister for promoting more open government.) It is not, to put it mildly, the custom for British ministers or civil servants to describe and discuss such hot political events, even unattributably, with academics or journalists. Quite possibly, only Butler's standing could have achieved this almost complete cooperation, granted that several major sources are now retired from ministerial or official service. It is particularly to Sir Terry Heiser's credit that he joined in as his feet are held to the fire. The authors note that he headed the relevant department (Environment) while also being a longstanding expert official on local government finance, rather than the usual generalist mandarin, who typically has little more personal experience of a current post's work than the temporary and "lay" minister placed in political charge of that field. They clearly conclude that this permanent secretary failed, at both administrative and technical levels, to do what he could to avert the hugely costly disaster of introducing—and then bailing out—the poll tax. In particular, he acquiesced in ministers' refusal to consult any practitioners or other experts in local government finance. In general, on this account, he and other very senior officials "went along," when the obvious novelty of the new tax should alone have elicited much more caution and a professional duty to warn.

Surely, such a dramatically controversial and novel tax must have arisen from a conscious, predictive "policy theory": "If *x* is enacted, then *y* will result and produce solution *z*." In fact, the authors' main finding is that the reasons for choosing the poll tax were largely negative and even unthinking. Mrs. Thatcher was much more sucked than propelled by any conscious campaign into endorsing it. She plumped for it as the only acceptable alternative to the long-established local property tax ("the rates"). The rating revaluations were coming due, which meant trouble from homeowners in Conservative constituencies, starting in Scotland, where the party was already on its knees. With a local income tax ruled out by the Treasury, the poll tax stood alone. More left-wing Conservatives claimed that it defied obvious common sense in charging rich and poor the same (granted that the officially poor would get part relief); but these critics included the hated former party leader, Edward Heath, the ideologically suspect Michael Heseltine, and Sir Ian Gilmour, whose opposition only strengthened the government's resolve.

As is now notorious, the poll tax was introduced (first in Scotland) and greeted with serious refusal, evasion, slow payments, demonstrations, and the worst London riot in years. Conservative fortune declined; Mrs. Thatcher became vulnerable, was wounded in the first party leadership ballot, and quit the field—probably more as a result of "her" poll tax than any other factors. This remarkable study shows that it was "hers" more from accepting it (having told her politician and civil servant courtiers "to do something—anything" to replace the rates) than from having either devised or promoted it. The trouble was, her word was law.

University of Essex ANTHONY BARKER

Strong Parties and Lame Ducks: Presidential Partyarchy and Factionalism in Venezuela. By Michael Coppedge. Stanford: Stanford University Press, 1994. 241p. $45.00.

Since competitive elections were restored in 1958, scholars generally regarded Venezuelan democracy as one of the most robust in Latin America. The political system was anchored in the emergence of an elite compromise supporting the democratic rules of the game (no easy feat considering the dictatorial legacy and the conflict-laden trienio), strong political parties, and a state-centered model of economic development that produced the highest gross national product per capita in the area. Venezuela's petroleum resources promoted not only economic development but also the consolidation of a democratic regime, through utilization of state resources to engender popular support and strengthen the two major political parties, Acción Democrática (AD) and Comite de Organizacion Politica Electoral Independiente (COPEI). Despite these impressive achievements, the political system became increasingly crisis-prone and experienced a loss of legitimacy with the onset of the debt crisis and the subsequent devaluation of the *bolívar* in 1983. Venezuelan democracy entered its deepest crisis during the second administration of Carlos Andrés Pérez, with food riots in 1989, two coup attempts in 1992, the impeachment of the president in 1993, and the failure of both major parties to elect a presidential candidate in 1993.

In this carefully crafted book, Michael Coppedge explains why Venezuelan democracy has become so fragile after three decades of apparent stability. Two political institutions are highlighted as the principal culprits: *partyarchy* and *presidentialism*. *Partyarchy* results from the two major parties' monopolization of the electoral process, domination of the legislative process, and blockage of many informal channels of representation. The author argues that Venezuelan partyarchy contributed to political stability (especially during the early years) but had the long-term effect of diluting the quality of Venezuelan democracy. Presidentialism, likewise, undermined political stability because of the frequent stalemates between the executive and legislative branches, which severely limited the president's ability to function effectively. Although neither partyarchy nor presidentialism by itself is "bad," both institutions together produced Venezuela's current political problems.

After delineating the nature of the political crisis, the author distinguishes between Dahl's concept of polyarchy and his definition of partyarchy, that is, "—the degree to which political parties interfere with the fulfillment of the requirements for polyarchy" (p. 19). He maintains that polyarchy is stifled in Venezuela because the major parties are not internally democratic and fail to offer voters a meaningful choice (p. 39). His argument is not entirely convincing, for no scholarly consensus prevails insisting that internal party democracy is a prerequisite for democratic government. Moreover, even though the policy differences between AD and COPEI have narrowed significantly, does this necessarily imply that voters are not presented a meaningful choice at the polls?

The study focuses upon AD's internal politics, drawing heavily upon 80 interviews with top party leaders in the National Executive Committee, Congress, and the Labor Bureau. Because the political process varies tremendously with the arena in which the party is operating, the author distinguishes three parties within AD: the policy party, the electoral party, and the power party. The pervasive factionalism in AD results not from policy, regional, or socioeconomic differences but from the struggle for leadership, linked to the five-year electoral cycle and a lame-duck president. The author convincingly demonstrates (in part, through a pooled cross-sectional time-series analysis of electoral data) that factionalism, which stems from the power party (ins vs. outs when AD is the governing party) detracts from the electoral and policy parties. Acción Democrática factions are transitory, fluid, and oriented toward selecting a winning candidate.

Coppedge has written what may well be the best book ever published on political party behavior in Latin America; it is extensively researched, methodologically sophisticated, tightly argued, and well written. Because the author demonstrates a mastery not only of the Venezuelan and Latin American literature but also the major theoretical works on party behavior, this study will appeal more generally to comparative and political party scholars. The work is particularly noteworthy in terms of linking political party behavior to the wider political system, along with suggesting policy changes to enhance the quality of democratic government.

But even more significantly, the book is bold and imaginative in the sense that it challenges prevailing scholarship on party behavior and undoubtedly will provoke controversy. For example, the literature generally suggests that strong political parties are critical for promoting democratic consolidation. Yet Coppedge argues precisely the opposite, at least in conjunction with presidential governments. His analysis also will stimulate further debate between those arguing the merits of parliamentary versus presidential forms of government. Coppedge's argument for curtailing presidential power in Venezuela hinges upon the high incidence of stalemate between the president and Congress, which is not well established empirically other than during the previous AD government.

State University of New York, Buffalo Gary Hoskin

Political Stability and Democracy in Mexico: The "Perfect Dictatorship"? By Dan A. Cothran. Greenwich, CT: Praeger, 1994. 272p. $55.00.

Dan A. Cothran is a new recruit to the rather large corps of interpreters of Mexican politics, but he follows squarely in the established mode. The problem in writing about Mexican politics is to interpret a large mass of rather contradictory data with a model that has internal logic, accounts for most of the data, and coheres with what we think we know about politics in general. Thus, in one version, the Mexican regime was, until recently, an authoritarian socialist system; and elements that did not fit into that interpretation were simply a facade for public consumption or were imposed by the necessity of living next door to the leading state in the world capitalist system. (Or vice versa: there are different versions of this essence-and-facade model.) Another possibility is to take contradiction itself as the essential feature of the system, which becomes one in which an elite maintains itself in power by pursuing policies that appeal to different interests even though these are policies that, taken by themselves, might be thought to derive from opposed ideological positions. This is fundamentally the approach that Cothran takes in the first two-thirds of the book, where he lays out his theoretical model. It is a good job, written in a straightforward unadorned style and marked by common sense and balanced judgement.

In addition to the elite strategies model, Cothran also explicitly endorses the modernization or political development model, which accounts for conflicting evidence by finding it to belong to different stages of development through which the political system is going, in response to a changing society.

Cothran has a fair knowledge of the literature, but with substantial gaps; and his grasp of Mexico's political history weakens from time to time. The real difficulty, however, comes with the recent period, in which the pace of change has accelerated and events have started escaping the control of the ruling elite, so that changes begun as tactical moves to accommodate conflicting pressures have acquired a momentum of their own. Here, the model of elite control referred to in the book's subtitle does not do the whole job of explaining events, as Cothran realizes. In the latter third of the book, in which he discusses the most recent period in Mexican history, no clear themes emerge, and contradictory tendencies are left unresolved and unreconciled. The situation is not helped by the fact that his shortcomings in knowledge are rather more frequent in this section of the book. This is ironic, since Cothran himself was clearly a witness of some of the events he describes, particularly the election campaign of 1988, and has more first-hand knowledge of this period. Apparently, one can be more fully informed and a more judicious commentator about events that have long gone—that have been much written about and analyzed extensively—than about more recent events on which no interpretive consensus exists. I think this would have been a good time for Cothran to have shown more confidence in political development theory and unequivocally interpreted the recent period as the supersession of the elite control model by the modernization model. Of course, modernization, like nostalgia, is not what it used to be. The modernized world economy turns out to be neoliberal, rather than a mixture along Scandinavian lines, and decision-making power seems to reside more with the bond markets and currency speculators than with national governments; but otherwise, those of us who discovered political development in the 1950s and 1960s turn out to have been not so wrong after all.

University of the Pacific Martin C. Needler

Encountering Development: The Making and Unmaking of the Third World. By Arturo Escobar. Princeton: Princeton University Press, 1995. 290p. $19.95.

The concept of *development* has played a core role in the theory and practice of economic change and political economy since 1945. Simply put, *development* refers to a broad pattern of macroeconomic structural transformation that, ideally, results in a shift of national economies from low-wage and low-growth systems. Although there has always been substantial disagreement about how such structural change can be effected, there is no doubt that at its best, development has been a focal point for all those who have hoped for a world order of stable prosperity and progress.

However, as Arturo Escobar's interesting new monograph, *Encountering Development,* shows in rich detail, the theory and practice of development has also been characterized by extraordinary errors of cultural bias, misunderstanding, and (ultimately) failed promises (pp. 1–4). It is Escobar's central thesis that there is no linear or universal model of economic and social development that can be objectively applied to the diverse local cultures of the societies misleadingly grouped under the heading of the Third World (pp. 1–17). Indeed, Escobar argues passionately that the construct of the Third World is an ethnocentric invention of the post–World War II West; and development is an equally flawed "regime of representation" crafted from a conflictual ensemble of ideology, partial interests, and the attempt of the West (esp. the United States) to impose its power-driven interests on non-Western peoples (pp. 10–11). In the end, development collapses as a unifying conceptualization of economic change—an outcome the author implicitly welcomes as the basis for an alternative "imagining" of multiple, non-Western paths of progress (pp. 212–26). This reconceptualization is supposedly necessary in order to contribute to the liberation and political visibility of non-Western peoples who have long been subjected to the constraining power of a complex developmental "apparatus" that has sought to make the "Third World" peoples subordinate and powerless (pp. 154–211, esp. 154–71).

If Escobar is even approximately correct, two enormous consequences follow for political science and political economy. First, it follows (if I have read Escobar right) that it is quite simply impossible to generate an empirically adequate theory of economic, social, and political change, owing to the incredible local diversity of ideas and interests Escobar posits as lying just beneath the mirage of uniform Third World peoples. Unlike its ecological counterpart, this sort of variation is evidently qualitative, rather than quantitative; and it is apparently sufficient to ensure that the application of a single, unitary theory to any particular case. Surely, should Escobar prove right about this, it must challenge the central belief of the social sciences that is possible to explain specific variations in terms of a comprehensive theory based on generalizable concepts and hypotheses. I am more than a bit skeptical about the problems supposedly inherent to general theory building; but Escobar's work is a powerful reminder of the need to subject our theoretical pretensions to a close engagement with empirical materials of all kinds, something Escobar has admirably done in the case of Colombia (pp. 123–53, 184–206).

The second large and provocative implication of Escobar's work is that the search for a model of development capable of practical implementation in poor countries is a chimera. Instead, the received model of development functions less to raise incomes and more to trap poor people in novel forms of discipline, surveillance, and oppression (pp. 1–20, 154–211, esp. 192–211). This largely unintended consequence is, Escobar seems to believe, the predictable result of the origins of development projects in struggles for power and intellectual hegemony in which the search for objectivity and effectiveness necessarily succumbs to the interests of the stronger, usually aid-donor, nations. For anyone who believes that it is possible to initiate efficient development programs in poor societies, Escobar offers a potent challenge and an important reminder that international beneficence may often mask more subtle and less reputable designs. Indeed, Escobar can be read as calling into question the very desirability of any form of developmental aid, even those that might be more congenial to the author's political perspective.

Considered as a whole, *Encountering Development* is a lucid and potentially important contribution to comparative social science, although many readers will find large parts of the book infuriating. Escobar has the courage to advance unconventional ideas without succumbing to the temptation of gross polemics, a virtue all too many who adopt Escobar's critical stance obviously lack.

Nonetheless, there are three not insignificant problems with this work that must be addressed. These have to do with the book's empirical claims, logical consistency, and broadly poststructuralist (some would say, *postmodernist*) method.

To begin with, Escobar occasionally advances sweeping empirical generalizations that have little evidential support and that I find difficult to square with the factual points an issue. For example, early on, Escobar claims boldly that "instead of the kingdom of abundance promised by theorists and politicians in the 1950's, the discourse and strategy of development produced its opposite: massive underdevelopment and impoverishment, untold exploitation and oppression" (p. 4). Escobar is undoubtedly right that development projects have often had the worst possible perverse unintended consequences, but does this generalization apply equally to all developmental experiments? As the cases of post-Mao China, Thailand, and even parts of Latin America and Africa indicate, the economic performance of non-Western countries has not been uniformly dismal since 1945, and it is difficult to believe that those countries' welfare and consumption are worse today that at the end of World War II. Nor is this claim an isolated instance pulled out of context. Instead, it is an example of Escobar's repeated use of large generalizations that are debatable at best (see esp. the discussion of peasants, women, and the environment that make up chap. 5). However, the real issue is not whether the author is right about this or that case in all particulars. Rather, the real point is that development may not be the monolith of theory and practice Escobar seems to make it, and it may just be that as a matter of fact, there are multiple development projects that have been more or less effective in generating growth. Ironically, this more disaggregated or "deconstructed" approach probably fits more effectively with Escobar's own healthy suspicion of unitary models and abstract generalizations.

A second problem in *Encountering Development* is the book's tendency—already alluded to—to mirror-image the logical pitfalls of the approaches Escobar eloquently critiques. This shows up, above all, in the author's rather uncritical use of categories like *race, class,* and *gender* as adequate representation of the life situations of people outside the West (see esp. pp. 182–92). It is not a matter of rhetoric or logic-chopping to point out that if the concept of development is inherently partial and ethnocentric, logic indicates that the same criticisms can be made of concepts like race, class, and gender. These, after all, are also derived from Western social science; and it is not clear to me why the latter constructs are any more effective than developmental theories in describing and explaining non-Western peoples. Similarly, although Escobar repeatedly attacks almost all theoretic generalizations, the book is based on generalization—not the least important of which is Escobar's identification of a single, monolithic theory and practice of Western-sponsored developments. It is difficult to see how Escobar can square these problems of consistency without either abandoning comparative concepts altogether or retreating into a position equally open to charges of bias. Neither alternative is acceptable, and it is to Escobar's credit that the book rejects jejune relativism in favor of an at least limited possibility of truth (pp. 19–20). But one is still left wondering why Escobar's categories and concepts work, while others do not.

Finally, *Encountering Development* is partly grounded in what seems to be a poststructuralist method, or (perhaps more accurately) *viewpoint* or *perspective*. It is difficult to see what this adds to the monograph; and fortunately, Escobar's work withstands scrutiny with or without its methodological luggage. However, the author's invocation of poststructuralist ideas (ultimately rooted in Foucault and his followers) raises a very troubling issue. As I understand them, poststructuralists deny the possibility that *any* intellectual enterprise can escape from the "violence" and bias supposedly inherent in the tendency of any idea or theory to become the basis for an effort to achieve power and cognitive hegemony, an effort that always ends by privileging some perspectives at the expense of others. But if this is true, where does it leave Escobar's search for a more humane and humanistic alternative to the developmental enterprise (esp. pp. 212–26)? Isn't any such alternative doomed to become, itself, another hegemonic enterprise in which new winners and losers will continue to engage in a ceaseless struggle for domination? This pessimistic conclusion is one that Escobar rejects; but it is a conclusion difficult to avoid, given the logic of poststructuralism, with its reduction of the world to an inevitable struggle for interpersonal and intersocietal dominion. Despite the best of inventions, Escobar's work seems, in the end, to leave us with no way out of the double bind of poverty and oppression.

Nevertheless, Escobar's *Encountering Development* is a book that should be read by anyone interested in critically rethinking the often facile hypothesis and constructs of comparative politics and political economy. It is a fresh vision and one that undoubtedly will have a real impact on our received wisdom. Above all, it forces us to think seriously about discarding our notion of a homogeneous "Third World" that can be reduced to simply explanatory formulas lacking in any concern for regional, local, and cultural variations.

University of California, San Diego VICTOR V. MAGAGNA

The Politics of Democratization: Generalizing East Asian Experiences. Edited by Edward Friedman. Boulder: Westview, 1994. 276p. $59.95 cloth, $21.95 paper.

The 11 essays in this book explore aspects of democratization in five East Asian political entities (Japan, Korea, Hong Kong, Taiwan, and the People's Republic of China) and suggest that the experiences of these countries might be "generalized" beyond the region. Most of the authors are themselves East Asians, holding positions both in academia and in international organizations. The editor is a recognized scholar of the region, whose democratization experiences include his having spent 1993–94 working on these issues with the U.S. Department of Defense.

The chapters, originally presented as papers at a 1990 conference, are generally well written and cover a range of interesting and important topics. However, there are notable gaps in coverage. For instance, Masanori Nakamura's chapter on the dynamics of democratization in occupied Japan (1945–52), the two chapters on Korea, and the one on Hong Kong all touch upon themes of how democratization is initiated and consolidated in specific political and economic contexts. One might expect these sorts of issues to be given comparable treatment with respect to Taiwan, but neither of the chapters on that country picks up on these themes to any appreciable extent. Instead, one considers the prospective relationship between peaceful resolution of Mainland-Taiwan conflicts and democratization in both countries, while the other provides a brief account of the Farmers' movement in Taiwan.

Two of the more interesting chapters are those written by David Arase and Yasunobu Sato, both dealing with the possibility of Japan's using its foreign relations, particularly its sizable *overseas development assistance* (ODA), as a force for human rights and democracy in East and Southeast Asia. In the short period since these essays were written, Japan has moved to incorporate into its ODA guidelines the very sorts of priorities and values advocated by Arase and Sato. The discussion of Japanese aid and democratization raises an equally important and interesting question not considered in this volume: To what extent have the democracy and human rights policies of other donor nations, particularly the United States, been a factor in the democratization of East Asian countries?

The editor provides an introduction, a chapter entitled "Democratization: Generalizing the East Asian Experience," and a conclusion—all of which purport to draw lessons from the case studies presented in the volume. Unfortunately, the generalizations turn out to be rather few in number and more accurately described as working assumptions than as conclusions drawn from the Asian examples. The core argument in all three of Friedman's contributions to the volume, as well as in Stephen Manning's on China, is that Western political thought has tended to conceptualize democracy in cultural terms that would deny even the possibility of democratization in Asia—a "categorical opposition of democratic Europe and despotic Asia" (p. 1)—and that the essays in this volume should force a rethinking of that perspective. Politics should be regarded as a more decisive factor in bringing about democratic development, culture, less so.

These are valuable and stimulating insights, but they fall short of the generalizations promised in the book's subtitle. By the time the same argument has been encountered for the fourth time, one wonders whether there might be other lessons worth drawing from the East Asian experience. The varied case studies would indeed yield several such lessons. Much more could be done, for instance, with the interplay of internal and external factors in democratization or the dynamics of reform and consolidation phases in democratic development, both themes touched upon in the essays but not drawn out in the introductory or concluding commentary.

Despite these criticisms, this is a book which both East Asianists and students and scholars of democracy should find worth reading and discussing. It is rich with assumptions concerning democracy and democratization, and much of the empirical description and speculation in the various chapters will continue to be valuable whatever courses democratization may take in the East Asian region in the future.

Kansas State University WILLIAM L. RICHTER

Bound by our Constitution: Women, Workers, and the Minimum Wage. By Vivien Hart. Princeton: Princeton University Press, 1994. 255p. $35.00.

What difference does a written constitution make? Vivien Hart poses this question with respect to minimum wages for women in the United States and Great Britain. Women's labor legislation has been the subject of continual scholarly attention at least since the ruling, in *Muller* v. *Oregon* (1908), that laws invalid for men were acceptable for women. Feminists discovered the issue at about the same time constitutionalists exhausted its possibilities. Like Sally Kenney (*For Whose Protection?*, 1992), Hart has taken this issue into new territory by doing a comparative study. The result is an important contribution to constitutionalism, women's studies, and comparative politics.

Hart has taken on a formidable project. First, she must persuade the reader that the question is answerable.

Other differences between the two systems (unitary vs. federal, parliamentary vs. separation of powers, etc.)—let alone differences in economic policy and political culture—militate against efforts to track the effects of any single factor. Second, the author must show that the answer matters. This book's weaknesses stem from Hart's failure even to perceive the first set of problems. The book's value lies in its unexpected—and unexpectedly significant—resolution of the second set.

The book's intellectual honesty does its author credit. Hart provides enough information to allow the reader to dispute her conclusions; and there is plenty. The American judiciary's sustained, obdurate obstructionism on labor legislation, represented by *Lochner* v. *New York* (1905) and *Adkins* v. *Children's Hospital* (1923), lends support to the conventional view that "a potential for adaptability characterized Britain; a propensity to fixity the United States" (p. 4). So do the effects of the judicial rearguard action on American labor law. *Lochner* led to *Muller*, with its emphasis on physical sex differences. *Muller* undermined both women's protection and women's equality, by providing an excuse to overturn a minimum wage in *Adkins* and by legitimizing any discrimination (however invidious) with a physical basis (however tenuous). By contrast, Britain's first minimum wage was gender-neutral and antedated the Fair Labor Standards Act (FLSA) by almost 30 years.

On the other hand, the respective histories of the minimum wage might convince a cynic that the comparison is between six of one and a half-dozen of the other. Britain had a minimum wage only from 1909 to 1993; the FLSA, enacted in 1938, may not survive another Republican administration. In neither country has the minimum wage amounted to more than three-fourths of the estimated living wage. In both countries women have comprised the majority of beneficiaries of the laws because women's average wages lag far behind men's. And Kenney's study of women-only workplace restrictions in the same two countries demonstrates that physical differences have been so less common an excuse for restricting women in Britain than in the United States.

But Vivien Hart insists that the Constitution did make a difference—and a positive one. She reminds us that the Constitution's effect on minimum wage law was not limited to hostile decisions. The Equal Rights Amendment, first introduced in Congress the same year *Adkins* was decided, generated valuable debate over—and reinforced the presumed dichotomy between—equality in law and equality in fact. American women "have been gainers" because "relative to Britain, their dual identity and double burden have been open issues" (p. 181). The constitutional tradition "enabled claims to be based on dignified grounds of right rather than demeaning terms of need" (p. 180). Hart gives this rights-based discourse a large share of the credit for the gradual expansion of the coverage to include 90% of workers, as opposed to 10% in Britain. The inclusion of farm and domestic workers effectively extended coverage to large numbers of black and Hispanic women.

Hart's awareness of racial issues is a welcome corrective to the race blindness of much feminist scholarship on labor legislation. Her argument that a relationship exists between the written Constitution and the greater inclusiveness of American minimum wage law, if not compelling, is an ingenious and perceptive explanation of a counter intuitive result. Why should a nation which has verged on a welfare state and has no nasty judges to thwart the legislative will lag behind a government as reluctant as ours to interfere with market forces? But Hart too readily accepts the dichotomization embedded in relevant discourse and scholarship. Her distinction between rights and needs, for example, ignores the arguments of contemporary feminist scholars that these need not be oppositions (e.g., Rothman's *Recreating Motherhood*, 1989). This weakness may be symptomatic of a tendency to rely on simple dichotomies as explanations of different outcomes. Yet even if Hart makes her subject simpler than it is, she has handled it in a provocative and insightful way.

Texas A&M University JUDITH A. BAER

Nigeria: The Politics of Adjustment and Democracy. By Julius O. Ihonvbere. New Brunswick: Transaction, 1994. 321p. $34.95.

The author argues that despite the collapse of the Soviet Union and the end of the Cold War, Marxist analyses of the Nigerian social formation retain their validity and that the capitalist (American) model for development does not provide a guide for leading Nigeria—or any other African country—out of its current condition of underdevelopment. Yet the promise to reevaluate the problems and prospects of Nigerian development in the light of current global changes is not fulfilled. In the end, the book reaffirms and repeats the standard condemnation of Nigerian politics argued by the Nigerian Left, including the earlier efforts by the author (e.g., Falola and Ihonvbere's *Rise and Fall of Nigeria's Second Republic, 1979–1983*, 1985), but with a softer edge to the polemic. Of course, recent historical material is added to the depiction of underdevelopment, structural exploitation, and political corruption.

The language is direct and pungent, sometimes vituperative, and frequently repetitive; the style of argumentation is journalistic; the analysis of the "problem which is Nigeria" is well known (but with a more relaxed Marxisant language and tone); and the alternative pathway to development is couched in the soft rhetoric of political reform. Ultimately, Ihonvbere can only hope that an emerging transethnic and transclass alliance and "unity among democratic and progressive forces in the country" will have the "careful and clever ability to exploit the productive and progressive aspects of both the capitalist and socialist ideologies anchored on the principles of democracy, accountability, social justice and the dominance of the will of the people" (p. 213). The shared misery of progressive groups brought on by structural adjustment policies has, paradoxically, created "some long-term, unintended prodemocratic political gains" (p. 147). He sees changes happening both in the reality and the intellectuals' interpretation of Nigeria. Nigerian political culture is shifting from a "blanket call for socialism (which is still important) to democracy, participation, human rights, employment, basic human needs, empowerment and basic freedoms" (p. 186). Socialism needs to be, and is being, deconstructed. The recommendations embedded in each chapter tend toward incrementalism and accept the "theoretical" status of bourgeois social science concepts—as in the argument that the World Bank must take politics seriously and impose "political conditionality" on recipient countries because without honest politics, there is no chance of economic development (p. 141), or the argument that the problems of Nigeria stem largely from a failure of personality and leadership.

The argument is laid out in chapters discussing the role of the state in underdevelopment and its floundering attempts to reconcile the "contradictions of accumulation and development" under international tutelage and capitalist thought (p. 19). Other chapters deal with Nigerian foreign policy in a declining economy, the conditions of Nigerian universities, the politics and impacts of structural adjustment, the possibilities of a transition to democracy, and the state of Nigeria in the 1990s.

There is also much ambivalence in the rhetoric, and it shows especially strong in the chapters on foreign policies, Nigerian academics, and the prospects for the future. Discussing Nigeria's efforts to help other countries or attract private investments, Ihonvbere seems alternately critical of the efforts yet also upset that they are done so badly. He argues, for example, that "it was necessary for Nigeria to move and assert its leadership in the subregion" when it intervened diplomatically and militarily in the Liberian crisis; yet the bloodshed continues, at great cost to Nigeria, since Nigeria "lacks the military, economic, and diplomatic power to resolve the crisis decisively" (p. 63).

The condemnation of academics and their corrupt, opportunistic, and sexist behavior goes squarely to the core contradiction of Nigerian intellectual life. (The chapter contains the first extended mention to date, by a male Nigerian, of the sexism rampant in Nigerian universities.) Academics do not teach nor care for their students; they do not conduct research nor know how to manage the affairs of higher learning; they commit "high level[s] of intellectual fraud and academic dishonesty" (p. 93). They are agents for the state and international capital; they have become self-interested and self-serving, looking only to survive and prosper by graft, ethnic chauvinism, and subservience to the state; the "vast majority of academics have no business being there" (p. 92). Universities "are no longer known for teaching and research but for politics, manipulation, ethnic and religious jingoism, petty squabbles, mediocrity, political posturing, opportunism, and the recycling of outdated, irrelevant and useless theories, methodologies and ideas" (p. 82). In many institutions, "the female students' hostels are nothing but brothels" made available to businessmen and military officers (p. 99). In sum, academics have abandoned their responsibility to work for the good and progress of the nation and to incite new generations to ideals of social justice and "genuine democratization" (p. 74). Their ideas are to be distrusted, unless they are progressive.

The dilemma is this: the author was part of this system until he left for the United States and he now writes from a distance. Ihonvbere acknowledges his participation (p. 106, n. 20) but argues that now, at least, he does condemn from within, rather than blame the problems of the university on structural and external conditions. The dilemma has multiple forms, most fundamentally that the author cannot practice what he preaches. He cannot help shape the Nigerian future by action (for he has left), nor can he develop the tools for others to use (for he cannot teach them). The book is an example of exactly the kind of research he thinks should not be done but is. There is little "real work" research in it; it is (like the typical papers and articles of Nigerian academics) an extended opinion supported by episodic facts, "impossible statistics" (e.g., "Over 98 percent of my former students who graduated with first or second degrees between 1989 and 1991 at the University of Port Harcourt remain unemployed today," and "There are more lunatics and prostitutes in Nigeria than ever before" [p. 107, n. 27, and p. 201]) and stylistic exaggeration (the Shagari regime "set the country back by over five decades," the Babangida, "by almost a century" [pp. 118, 198]). The interpretation offered is not based on the careful marshaling of facts but is supported mainly by citing other interpretations, most of them by journalists. His description, then, fits Nigerian public political culture more than Nigerian realities.

The book is an excellent example of its genre. The author knows what he is talking about and presents the evidence in vivid prose. The book is one-sided, partial, inveighs with sadness against imagined "traitors" (former radicals and liberals who have joined the state), and is fun to read. The book captures both the flavor of much intellectualizing in Nigeria, as well as the "realities" of Nigeria perceived from a "progressive" perspective. It adds little to our understanding of the Nigerian condition (for that interpretation has been offered before) but much to our view of the travails of Nigerian intellectuals.

Washington State University OTWIN MARENIN

Dynamic Models of Conflict and Pacification: Dissenters, Officials, and Peacemakers. By David Kowalewski and Dean Hoover. Westport, CT: Praeger, 1995. 184p. $59.95.

Dynamic models have persisted as a significant minority among the rational models that dominate formal approaches in the study of conflicts between governments and oppositions. Kowalewski and Hoover's work is the latest contribution to these action-reaction models of repression and dissent. It offers a general model that is applicable to a surprisingly broad range of literature. The authors forgo analytical estimation of their model and use simulation to yield implications for research on the war-dissent linkage, charismatic leadership, elections, vigilantism (chap. 6), conflict resolution techniques (chap. 7), and new social movements (chap. 9).

The basic model (chap. 2) is a system of equations that connect the rise and fall of the grievances and resources of movements and regimes with the intensity and scope of dissent and repression. The model design allows the authors to focus on the functional relationship between dissent and repression: each actor can choose a "strategy" of being sensitive (positive relationship), imperturbable (no reaction), or opportunistic (negative relationship) in response to changes in their rival's level of hostile activity. While their survey of 101 existing empirical findings indicates that regimes and movements are usually both sensitive, they use their model to formulate generalizations about the conditions under which regimes, movements, conflict-resolutionists, and (most interestingly) particular factions within each group might wish to pursue one strategy or another.

The main strengths of this book lie in the model's innovative ways of examining issues that have been neglected by others. The model is based on an integration of grievance-, resource-, and opportunity-based theories of dissent. This allows variables such as regime grievances to be given needed consideration and leads to a useful distinction between intensity and scope of repression/dissent. Most valuable is the focus on factions (10 each for the movement and regime), along with conflict resolutionists, which can also consist of factions. By abandoning a unitary-actor assumption, the authors can infer useful strategies using a dynamic modeling structure that is often criticized for its neglect of strategic interaction.

Another strength is the attempt to analyze a system with more than one opposition movement (chap. 8). An interesting finding here is that dissent decreases when there are two movements, rather than one. Also, by using different reaction strategies against each movement, the regime may drive a wedge between the two movements; moreover, it can shape the factional makeup of the opposition by varying its response strategy. Other interesting findings in the book include the effectiveness of binding arbitration as a resolution tactic (as compared to four others); the strong dampening effect of elections on the scope and intensity of the conflict; and the specification of optimal strategies for each faction in the movement and regime, which would allow prediction of coalitions and resulting strategies if applied empirically.

Unfortunately, several problems with the book make these findings hard to discover and interpret and limit their usefulness to the nonmodeler audience who might benefit. A main weakness of the book is something that has made dynamic models peripheral to this area of research for too long—a lack of attention to cumulation. The authors do not explicitly build on previous dynamic models of conflict, and they do little to integrate their findings with those of rational and game-theoretic models, which are more prevalent in the literature (e.g., DeNardo's *Power in Numbers*, 1985). By synthesizing psychosocial, economic, and political explanations for civil conflict, they forgo the opportunity to settle theoretical debates between these perspectives. Most of the findings are presented without empirical illustrations or even anecdotal evidence, which makes them hard for the reader to interpret and evaluate. This makes the book arduous to read, even for modelers. The authors use a large amount of new terminology, especially for the types of factions in each group, which is difficult to apply intuitively to existing conflicts. One or two brief case studies that illustrate the processes and findings proposed by the model would make this work much more useful to a wider audience and would give readers more confidence in the validity of the model.

A further problem is the lack of attention to justifying the construction and parameterization of their model. While some obvious links exist between its functional form and other action-reaction models, these are not elaborated. Furthermore, the parameters that determine how much one variable affects another are not defended; we are told that these are based on the "findings in the empirical literature" (p. 20), but no specific findings are discussed. The parameter levels are not unreasonable, but I suspect that small changes in their levels would affect several of their findings. Why not simulate the system using several plausible parameterizations and look for patterns? The authors explicitly ask for the reader's faith: "The greatest utility of dynamic modeling lies in discovering what happens *if* the parameters *were* true, thereby enabling us to explore the consequences of our assumptions" (p. 133). Yet they should do more to convince the reader that their parameters are at least *likely* to be true before proceeding.

The reader is left with some interesting findings but also nagging doubts about the theoretical and empirical validity of the model. The authors' analysis of their basic model (chaps. 3 and 4) begins to persuade us that its processes are reasonable, but this would be strengthened by adding a theoretical justification of the parameters or empirical illustrations of the processes and findings. Dynamic models are powerful tools for analyzing conflicts, and this work has shown that they are flexible enough to account for factions, third parties, conflict resolution attempts, and many other necessary complications in ways equaling or exceeding that of other models. If more attention is paid to linking these models' design and findings to existing theoretical and empirical literatures, then they will get the attention that they deserve.

Bowling Green State University MARC V. SIMON

Hausaland Divided: Colonialism and Independence in Nigeria and Niger. By William F. S. Miles. Ithaca: Cornell University Press, 1994. 368p. $49.95.

Investigating behavior in two neighboring Hausa villages (each located in Francophone Niger and Anglophone Nigeria), the author presents a case for the continuity of colonial institutional norms in postcolonial Africa. European colonialism has made a difference in the lives of African peasants long after the departure of imperial detractors. The borderline has assumed politicomagical significance as an otherwise artificial border is "invested with special powers" (p. 302). The author's main thesis is that "the colonial partition, though external in origin, has become an internalized, commonplace reality for millions of borderline villagers throughout Africa" (p. 19).

The book comprises 12 chapters. The first and last are more theoretical and analytical. They focus on border problems, and advance several major hypotheses associated with such problems; chapters 2–4 introduce the reader to the domestic sphere of intra-Hausa interactions; and chapters 5–11 incorporate the dimension of the continuing influence of British and French colonialism upon the local setting.

Chapter 2 describes, in addition to the villages' histories, internal divisions and food security, why the author chose the particular villages he does: Yekuwa (in Niger) and Yardaji (in Nigeria). Chapter 3 evaluates the nature of the Hausa ethnic identity. Yekuwa residents express higher national consciousness. Being Hausa is slightly more salient in Yardaji. Significantly, "in both communities greater affinity is stated for non-Hausa co-citizens . . . than for 'foreign' Hausa" (p. 57). Chapter 4 discusses border matters, including cross-border contacts. Miles notes that "middle-level elites [not peasants] are more likely to find themselves subjected to the scrutiny of border and security agents" (p. 85). His argument that independence has not resulted in greater interaction between neighboring African administrators is not well supported by the literature (Mazzeo 1984; Onwuka and Sesay 1985; Zartman 1987).

Chapter 5 examines how the different colonial policies affected Hausaland. The British established their presence in northern Nigeria with general ease, whereas the

French practiced a harsh regime. Niger, not Nigeria, retains a strong military culture: "Hausa villagers from Niger are struck by the lack of respect accorded to authority in Nigeria. People would never insult Kountché in Niger, claimed one; yet many Nigerians openly insult their head of state" (p. 113). Chapter 6 analyzes archival records of colonial policy differences, and chapter 7 contrasts traditional rulers in Yardaji and Yekuwa, revealing significant disparities in status and influence. The chief's residence in Yardaji has greater prestige. Readers may charge the author with articulating a self-fulfilling prophecy where colonialism explains everything. Cannot the differences in the status of the chiefs be attributed to economics? Miles argues that economics matter more among Hausa villagers where there is robust activity on the Nigerian side (chap. 8).

Chapters 9 and 10 review the impact of colonialism on social evolution in rural Hausaland. Colonial patterns of education persist with astonishing continuity. Teachers enjoy greater renumeration and status in Niger where an elitist French colonial schooling has produced more efficacious schools even if curricula and textbooks inadequately reflect local culture. On religion, where "the British showed toleration, the French exhibited distrust; where the British allowed a measure of autonomy, the French strove to control and contain" (pp. 249–50). Chapter 11 contrasts village culture in the two communities and concludes that the ideal of democracy is deeply ingrained among Nigeria's "elites, even the military": "Yekuwa's 'democratic experience' has been quite different. Only relatively late in the colonial era did the French accept even the principle of democracy for subjects of French Hausaland" (pp. 276, 278).

Miles revisits, in chapter 12, questions of boundary persistence and national identity and asks why "colonial boundaries persist with such great force, even after the hegemons that originally drew them have disappeared from the scene" (p. 304). He offers six explanations: (1) Hausa elites who maintain a material stake in perpetuating existing boundaries; (2) people who do not appreciate the linkages between postcolonial politics and colonial oppression; (3) inherited colonial institutions that have acquired legitimacy; (4) citizens who desire to elevate their nation-states to the status of the global community; (5) interacting indigenous communities which tend to highlight idiosyncratic differences; (6) the postindependence generation that has acquired fresh affective, evaluative, and cognitive political orientations.

The book is an important contribution to comparative colonialism and borderline studies. Citing similar cases in Europe and the fragmented Soviet Union, the author's preoccupation and claim is universal. His concern goes beyond forecasting boundary instability in West Africa. His methodology is empirical and indigenous. The data speak for themselves, leaving behind a priori notions of how French versus British colonialism should be conceptualized and concentrating, instead, on the way it is perceived by the inheritors of the partition. A major problem remains the extent to which valid generalizations can be drawn from the case study of two neighboring villages, either about comparative colonialism, African politics, or border problems.

It is hard to know where the author stands with respect to the fate of Africa's borders. While supporting Africa's right to have them removed, Miles maintains that the borders are here to stay. Noting the growth of transnational relations and interdependencies outside Africa, the author inaccurately contends that African leaders are disinterested in matters extending beyond their inherited colonial boundaries: "It is not clear . . . that the rulers of the new states have regarded their boundaries in new ways. A fresh look at these inherited colonial demarcations is needed, not with the aim of dismantling them but rather with a view toward transcending them" (p. 308). Nowhere in the study does the author acknowledge the various efforts of African leaders to pursue regional cooperative ventures especially within the context of the Economic Community of West African States.

University of Arkansas, Little Rock MICHAEL O. ANDA

The Failure of Political Islam. By Olivier Roy. Cambridge: Harvard University Press, 1994. 238p. $22.95.

A view of Islam as a religion of political extremism, containing the seeds of its own politicization in a manner inherently incompatible with much of the Western world, has gradually become well entrenched in Western policy circles. Unfortunately, scholarly writing on Islam has frequently contributed to this perception. Assuming that the strength of Islamist movements lies in a broad popular commitment to recapturing an idealized historical past, scholars have written of the "revival" or "resurgence," of Islam, situating current political movements as a return to this past, a reclaiming of the essential core of what is "truly Islamic." Ironically, this approach resembles nothing more than the rhetorical claims of politicized Muslim groups themselves. In this erudite and powerful book, Olivier Roy persuasively challenges such ahistoricism. Roy, whose earlier work, *Islam and Resistance in Afghanistan* (1986), remains one of the best available works on that subject, uses his wide-ranging and detailed knowledge of the Muslim world to present a reading of contemporary Islamic movements that provides an important corrective to such gross simplifications. The argument is all the more pertinent at a time when well-known American political scientists see global politics reduced to a "clash of civilizations."

To speak of the relationship between Islam and politics in general, "as if there were one Islam, timeless and eternal," is, for Roy, both "intellectually imprudent and historically misguided" (p. vii). He thus limits the focus of the book to contemporary Islamist movements, "the activist groups who see in Islam as much a political ideology as a religion, and who are therefore breaking with a certain tradition themselves" (p. vii). Such groups are the sort that took control of the state in Iran, participated in challenging Soviet power in Afghanistan, and seem on the verge of victory in Algeria. And these, he insists, are a distinctly modern phenomenon, which must be distinguished from other political tendencies of the Muslim world. While Islamism is clearly rooted in the conservative fundamentalism of the traditional scholars, the *ulama*, it is also distinguished from it in various ways.

Their attitude toward three areas in particular distinguish Islamists from fundamentalists: revolution, the *sharia*, and women. While fundamentalism seeks first to Islamize society, assuming that an Islamic political order will then follow naturally, the Islamists have embraced the notion of revolution, arguing that the assumption of state power will be the means of ensuring the Islamization of society. In contrast to the fundamentalism of the *ulama,* dedicated primarily to the effort to maintain and apply the inherited legal dogma of the *sharia,* Islamists have tended to give preeminence to the political logic of their goals. And while fundamentalist Islam insists on the careful circumscription of the role of women, Islamism has insisted on women's taking their place in society ("appropriately" attired, it is true) as militants, students, and workers. Saudi Arabia is thus the prototype of the fundamentalist state, Iran the model of an essentially Islamist political order.

And as the case of Iran demonstrates, Islamism's other roots are in another political tendency of the Muslim world—antiimperialism, more recently transformed into anti-Westernism. Islamism is thus a highly modern phenomenon, a product of a specific historical moment in the Muslim world. While its ideological precursor was the Salafiyya movement of the late nineteenth century, whose demand for the right of individual religious interpretation (*ijtihad*) effectively broke the *ulama*'s monopoly, Islamism is ultimately a twentieth-century phenomenon. Its major ideologues, the Egyptian Hassan al-Banna and the South Asian Abul-Ala Maududi and their respective organizations (the Muslim Brotherhood and the Jamaat-i Islami) spawned all subsequent Islamist movements that have marked the region. But for Roy, it is also a force whose power is largely spent: "Islamism was a moment, a fragile synthesis between Islam and political modernity, which ultimately never took root" (p. 75).

The recognition of this "failure" of Islamism is in no way a denial of the enduring power of Islamic political movements. Islamic groups may continue to seize power in various countries (an outcome Roy thinks likely in, e.g., Algeria); but their failure lies in the fact that they are incapable of fulfilling the promise of their rhetoric, of inventing a "new society." And the recognition of this failure has, in turn, gradually eroded the power of Islamism, leading instead to a synthesis with earlier ideologies. In successive chapters the book explores both the ideological and sociological sources of Islamism, discusses the "impasses" or inherent limitations of Islamist ideology, and chronicles its drift into a form of what Roy calls *neofundamentalism.* It is, in fact, this offspring of Islamism that he thinks likely eventually to win power in Algeria. These chapters provide, in effect, an ethnography of Islamism; much attention is devoted to exploring the "mind-set" of the movements, often based on a masterly reading of the literature produced by Islamists themselves.

Turning to the intricacies of current politics in the Muslim world, Roy is no less impressive in the richness of the evidence he brings to bear on his argument. Chapters on Islamic economics, geostrategic politics in the Middle East, and the cases of Afghanistan and Iran all underline his main point: rather than achieving the establishment of Islamic states or Islamic societies on a new model, Islamist political action has done little to challenge the prevailing logic of international political rivalries, state organization, twentieth century "Third Worldist" economics, or preexisting modes of social segmentation.

The book thus historicizes and contextualizes Islamic political movements, noting the historical evolution of the relationship between Islam and politics and identifying the forces of change, as well as distinguishing movements and tendencies from each other. This historical approach provides a basis for understanding the ways in which Islamism has reshaped the political landscape of much of the contemporary Muslim world, leading to the apparent "revival" of Islam that has marked virtually every state in the region: "The enfeeblement of Islamism into neo-fundamentalism brought an end to the revolutionary dynamic but contributed to the popularization and legalization of re-Islamization" (p. 129). In a grim concluding chapter, Roy forecasts the effects of this evolution. The neofundamentalism that now dominates the region is for him an "Islam of resentment," directed against the perceived Westernization of society, but in fact equally a rejection of Muslim civilization. It aims instead at the creation of a puritanical society, doomed, as all puritanism is, to hypocrisy and boredom. Only the family, whose privacy in the Muslim world is protected by the *sharia,* provides a space for retreat and leisure. But ironically, "what is circulating in the urban family is the opposite of the Islamic way of life: it is a product of the West. There are no 'Islamic leisure activities'. The new cultural models (videos) are blossoming into the very heart of the Islamic identity" (p.196). The failure of Islamism is complete.

The Failure of Political Islam is in many way an extended essay, persuasively presented by a scholar who brings a vast knowledge of the subject to bear on his argument. Ably translated from the French (by Carol Volk), it nevertheless retains some of the hallmarks of French scholarly discourse; and non-Islamicist American readers may find the writing just a bit elliptical at times. But it is a forceful work—an eloquent contribution to an important current debate by a scholar with long experience in the world of political Islam.

University of Kansas LEONARDO A. VILLALÓN

Evangelical Christianity in the United States and Great Britain: Religious Beliefs, Political Choices. By J. Christopher Soper. New York: New York University Press, 1994. 244p. $40.00.

J. Christopher Soper's examination of evangelical Christianity and the role of its ideology in social movement formation and activity is a useful addition to the literature on religion and politics and social movement theory. Surveying the theories of social movement formation, Soper examines psychological or status, rational choice, resource mobilization, and state structure theories of collective action and finds them all wanting in their attempts to explain evangelical political action in Great Britain and the United States in two distinct time periods. Soper then addresses evangelical ideology and group formation and postulates one of his central arguments—that it is ideology that is the driving force behind evangelical political mobilization. This is done in a thorough and well-documented manner, leaving the reader with little doubt that this is indeed the case.

The next two parts of the work examine, in detail, evangelical political mobilization in Great Britain and the United States, during the temperance battles and, later, the battles over abortion, pornography, and religion in state-supported schools. The comparative nature of the study and its focus on contemporary and historical battles seem to solidify Soper's argument.

Having clearly indicated the role that ideology plays in social movement formation and activism, Soper makes his second central argument—that political structures in place in particular states largely determine the political outcome of social movement battles. For instance, Soper argues that ideology provides the necessary motivation but that state structures and political institutions influence the ability of these movements to affect public policy outcomes. Thus, he argues that the federal organization, weak political parties, and open nature of the United States polity makes it easier for social movements to penetrate and achieve some successes. Yet the unitary nature and strong political parties of the British polity make it far less likely for interest groups and social movements to achieve their ends politically. Hence, a chief reason why evangelicals have been moderately successful in the United States and not in Britain is due to the structure and openness of the political system in which they must operate.

Soper's book fills an overlooked gap in the literature on religion and politics in that it is both a comparative and historical study and a study applying a different theory of social movement mobilization to evangelical Christian movements. While there has been a good deal of work in recent years on evangelicals and politics, particularly in America, there has been little work that examines evangelical political activism from an historical or a comparative perspective. This is the chief contribution of this work.

Soper spends a great deal of space debunking four prominent theories of social movement mobilization in the case of evangelical Christian movements and emphasizing the role of ideology in group formation. In doing so, he argues that existing theories do not take religious beliefs into account in their explanation of politically motivated activity. That may be true for that branch of sociology that deals squarely with social movement mobilization; but scholars of religion and politics in numerous conference presentations and some published works have noted, for several years, the important role that religious beliefs play in the motivation of activists. Soper's first argument therefore—that ideology leads to political activism—while not stunningly original, does indeed present detailed empirical verification for this theory.

One of the work's shortcomings is more of a misnomer than a true problem with the work. The author indicates that the work is concerned with evangelical Christianity, yet it really focuses only on *conservative* evangelical Christianity. Other than one brief and passing mention of the more liberal evangelical Christians and their often very different political objectives, the book does not address them at all.

Overall, this is an informative and interesting work and a worthy contribution to the literature of both political science and sociology. It would make a useful addition to library and personal bookshelves.

Middle Tennessee State University LISA LANGENBACH

Transition to Democracy in Latin America: The Role of the Judiciary. Edited by Irwin P. Stotzky. Boulder: Westview, 1993. 401p. $59.95.

A deeply rooted democracy requires a judiciary fully committed to due process, liberal principles, and the protection of individual rights. Both descriptive and theoretical analyses of transitions to democracy in places like eastern and southern Europe, Latin America, Africa, and Asia, however, generally neglect the judicial system. They tend, instead, to emphasize the importance of elections, political parties, civic culture, the behavior of civilian and military elites, and economic performance. Courts and judges—and other judicial personnel and structures, as well—are unquestionably critical to democratic reform. Stotzky's book thus successfully focuses our attention on issues fundamental to serious scholarship about transitions to democracy.

As a collection of essays, this volume contains 26 chapters including the editor, Irwin P. Stotzky's, introduction and conclusion. They are appropriately organized into six topical groupings: judicial roles in democratic transitions, judicial independence, trial as a judicial process, the role of the prosecutor, and judicial review and remedies. Within each part, one finds some excellent analysis of judicial politics within the complexities of democratic reforms. For example, judicial independence is well emphasized; this section's essays make clear the legal issues involved and their potential consequences. Some present the dangers of a nonindependent judiciary. Other authors, like Jorge Correa Sutil, create an enlightening tension with the conclusion that one should be careful of independence. In this manner, issue clarity successfully comes through contrast. Thus, through some very clear argumentation, these essays frequently raise important—even fundamental—questions about the judiciary in transitions to democracy. The volume, however, contains differences in quality of the various essays and strains to find a common thread that binds the pieces together.

One cannot help but question the logic for including many of these essays. The chapters by President Jean-Bertrand Aristide of Haiti and the former president of Argentina, Raul Ricardo Alfonsin, appear to serve other than scholarly purposes. A few chapters are first-person narratives. Furthermore, in an apparent attempt to include many different authors, the volume's structure appears to have necessitated short chapters. Cursory analysis and shallowly developed intellectual discourse tends to result, even though many profoundly interesting topics and issues are broached. And many essays stand strangely in isolation. For example, a single article from a feminist perspective sets a distinctive tone when its author states that "to talk about universal patterns and objective reasoning is simply male language" (p. 165).

The book's title is a misnomer. The analysis is not clearly focused on Latin America. In terms of specific cases, this book has more to do with the United States. (One oddly juxtaposed chapter even makes Britain its subject.) Many of the chapters are written by American law professors with examples drawn only from the United States. Some clearly know very little about Latin America. For example, one chapter is a fictitious opinion by a fictitious judge on the Supreme Court of Argentina. Toward the end of the opinion, this judge questions:

> Can I, a bogus and alien judge, have anything relevant to say about the opinion-writing functions of the Argentine judiciary? . . . I have drawn upon Anglo-North American, not Latin-South American, sources for the canons. Even my reading of the Argentine Constitution has relied on an English translation. I do not know Spanish. I know almost nothing about the people of Argentina, about the history of their country, and about their life, their customs, and their legal culture. (pp. 305–6)

Whether intended as irony or not, such historically underdeveloped writing unfortunately epitomizes this volume's tone.

Some excellent arguments of legal-political issues are nevertheless presented, for example, Robert A. Burt's "Judicial Independence and Constitutional Democracy: Lessons from the U.S. Experience." Unfortunately, the reader is frequently left alone to apply such lessons and examples to Latin America. Of those chapters which do cast their analysis within the Latin American context, Argentina is, with only a few exceptions, the primary focus. Argentina is indisputably an important case. The volume nevertheless does little justice to the rich variety of Latin America. Readers with such a comparative interest will likely find that the description and analysis of this region falls short. They may also question whether the United States, judiciary and legal system should be held as the only beacon of hope for countries making the difficult transition to democracy.

A dearth of theory contributes to the difficulty in drawing comparisons and applying U.S. lessons to Latin America. Little beyond the book's general organization links the essays. While a few individual chapters raise important issues about the law and judicial behavior, theoretically, they are at best spotty and chapter-specific. Despite some clear arguments, the lack of theoretical focus makes comparison problematic.

Some interweaving of the articles' themes does occur but in a rather strange and underlying manner. Left unexplained, many chapters appear to be direct responses to others in this same collection. Some uneven prepublication reading apparently went on; many contributors make references to other essays in the volume. The lack of any standardized presentation style makes it extremely difficult for the reader to do the same. The absence of an index does not help either. More annoying, many essays allude to the contents of "unpublished manuscripts on file with Irwin P. Stotzky" (e.g., p. 166).

Finally, a shortage of empirical evidence dominates this work. Most of the authors present little systematic evidence to support their claims. Statements potentially of interest to many scholars are left unsubstantiated and undeveloped, for example, "In recent years, the largest number of political killings have occurred in countries whose political processes are democratic" (p. 251). Clear argument is one thing, but the inability to refute assertions on empirical grounds is another. A scarcity of data also characterizes the essays' frequent calls for reform. Even sympathetic readers will remain skeptical on factual grounds.

In many ways this book represents a "missed opportunity." The role of the judiciary tends to be neglected in the literature on transitions to democracy. To its credit, this book reminds us about the importance of the judiciary in successfully implementing democratic reforms. It should not, however, be emulated by future works on the judiciary in regions such as Eastern Europe. They would be better served by drawing from other books and making comparisons with other cases, such as the successes and failures of Weimar Germany and contemporary Spain. Most importantly, however, future volumes on the role of the judiciary in transitions to democracy should spend more time questioning their goals, format, and style.

Emory University THOMAS D. LANCASTER

Power in Movement: Social Movements, Collective Action and Politics. By Sidney Tarrow. New York: Cambridge University Press, 1994. 251p. $59.95 cloth, $17.95 paper.

The brilliance of this book is the author's ability to transcend conventional schools of social movement analysis and to propose a framework tying movement emergence, development, and decline to broader social and political processes. Sidney Tarrow accomplishes this transformation by redefining the primary challenge to movement success as the management of social action rather than as overcoming individual reluctance to contribute to the attainment of collective goods. Tarrow suggests that social expression is natural in circumstances in which individuals share a common purpose. The movement dilemma then becomes one of "coordinating, sustaining, and giving meaning to collective action" (p. 16). Tarrow shifts the focus of analysis from the individual to the collective. In Tarrow's theory, inciting and then managing movements is the challenge, rousing individuals from inertia to collective action is a secondary concern.

Sidney Tarrow argues that even if movements are, to some extent, preordained, they arise within specific societies in identifiable time periods. They form, endure, and exercise influence based on their ability to adapt and respond to political opportunities, develop or borrow repertoires of contention, link themselves to social networks, and appropriate cultural frames of meaning and symbols. This suggests that they are important political actors as well as being acted upon. Movements draw selectively on internal resources, such as networks of allies and tested methods of challenging authority, as well as external factors including times of political opportunity and social symbols which serve the movement's purpose. Tarrow uses this complex model of social movement development to examine why particular decades give rise to large clusters of movements and why movement behavior varies from period to period.

On a macro level, he argues that the nature of social movements has changed considerably across the decades. Tarrow dates the beginnings of modern movements from the eighteenth century France, Britain, and America. During that century, movements changed from more fragmented and private forms of action to more public and state-centered manifestations, developing new methods of gaining political influence in the modern nation-state. Because movements now had a common target, the state, rather than the disparate antagonists of earlier periods, including, grain dealers, landlords, tax collectors, and merchants, they developed repertoires of pressuring for change that in Tarrow's terminology became "modular." It was possible for movements to borrow techniques of collective action from one another and apply them to achieve diverse goals. This modularity of techniques, diffused through the widespread availability of printed materials, meant that clusters of movements became the norm rather than the exception. As Tarrow traces this trend into the Twentieth century, the prevalence of television, satellite communication, and, more recently, e-mail and faxes speed the diffusion of social movement behaviors. The contrast between the cross-national spread of movements in the nineteen sixties, including feminism, environmentalism, and peace activism, with the lightening spread of movements advancing democracy in the late

1980's, shows the potential for technologies to change global diffusion of movements.

The concept of political opportunity is central to *Power in Movement*, but rather than representing an external factor which signals "go" or "no go" to movements, opportunity is used as a dependent as well as an independent variable. Movements with strong social networks, powerful symbols, and sufficient resources may seize and exploit even modest opportunities. When several movements successfully emerge and challenge government, the range of opportunity is often widened for other movements. The tactics of movement politics (e.g., their potential for innovation, their adoption of symbols and stories which carry meaning in their societies, and their borrowing of repertoires of contention from the experiences of unrelated movements) make them initiators of change instead of lurking presences awaiting social disorder to emerge and challenge government. Still, when opportunities are plentiful, more movements will break through, demanding change.

This book offers a rich mix of historical interpretation and dynamic integration of movements into normal politics. The theory of movements, as both guided by leaders and dependent on circumstances for their success, fits into contemporary experience. Similarly, the puzzles of why movements cluster in particular periods and why cycles of protest politics seem to follow one another at ever-shorter intervals find answers in this interpretation. But, although this book illuminates a great deal that was previously clouded about social movement behavior, there are areas which remain mysterious. Why individuals are motivated to participate in movements and whether this motivation has changed over the centuries is largely unexplored. With the creation of a global village, it is easier to identify like-minded people and foster international movements. There is a pattern of attraction to certain types of ideas and associations labeled identity politics by European scholars. Tarrow seems more comfortable with the concept of political culture shaping the opportunities available to movements than with analyzing the meaning of movement identities. He points out that ties deeper than social class currently seem to form the basis of the most intense movements in the world. It is ethnicity, race, gender, and even extreme nationalism which have powered much of the current wave of movement politics. In some ways, these ties seem to war against the global sweep which carries movement tactics and alliances across national boundaries. Might movement politics itself carry seeds of its own destruction, or at least limitation? If clannish, parochial ties ignite movements, which themselves use cross-national tactics to assail internationalism, contemporary movements may be self-limiting.

Another issue which invites further exploration is that although networks and allies are persuasively credited with facilitating movement emergence and growth, this book does not look seriously at the interdependence and opposition of movements to one another. It is possible that exploiting the opportunities created by other contemporary movements and borrowing the successful tactics of leaders can be done in a coordinated way rather than more randomly. Clusters of movements may learn to work together to magnify their opportunities for success.

Of far more significance than these questions is Sidney Tarrow's contribution, integrating movement politics into our larger view of politics in developed countries. This book moves us away from many of the questions about movements which, in retrospect, seem far too limiting. Rather than focusing on why individuals join movements, or whether resources, collective discontent, or political processes are most responsible for movement success, Tarrow presents a dynamic model in which movements are agents of change in environments providing different levels of political opportunity. Movements may arise and thrive through internal or external resources. The political culture, nature of the times, available communication technologies, and the presence or absence of other movements shape success in an environment which is continually changing. It is difficult to see movements in the same light after reading this book. *Power in Movement* captures the dynamism of movements while situating them in the political frameworks of North American and European societies.

University of Colorado, Boulder ANNE N. COSTAIN

Women and Revolution in Africa, Asia, and the New World. Edited by Mary Ann Tétreault. Columbia: University of South Carolina Press, 1994. 456p. $39.95.

This book's title understates its value, because it is about much more than women and revolution. To explain women's participation in Third World revolutions, we must first understand the gendered nature of their roles in the prerevolutionary social order and the ways in which that order is disrupted by the convulsive changes accompanying a nation's entry into the global political economy. By addressing these questions, this book reveals as much about gender inequities in the development process generally as it does about gender differences in revolutionary participation. Because the flash point of revolution is the temporal focal point of the case studies, the gendered nature of social institutions is more starkly revealed because those institutions are collapsing amid revolutionary violence. As such, this book is not only a much needed addition to the literature on women and revolution. It is an enlightening integration of feminist perspectives on development into the conventional literature on development and revolution.

Mary Ann Tétreault's opening chapter presents a substantively rich and theoretically rigorous framework with which to analyze the prerevolutionary status of women, the variables that affect the extent and forms of their participation in the revolution, and the payoffs they receive (or do not receive) in the postrevolutionary order. Her theoretical framework is well grounded in existing theories of development and revolution, with feminist theory thoughtfully integrated in such a way as to highlight those issues of gender that are largely ignored by mainstream theories of development and revolution.

Tétreault highlights the critical distinction between the private space of home and family and the public space of the economy and politics. She argues that women's biological role in human reproduction has relegated them to "the private subsistence economy of the household," restricting their opportunities in the public sphere. The prevailing ideologies and institutions of the male-dominated public sphere reinforce the gender-based division of labor by erecting additional obstacles to women's entry into the market economy and the political arena. The pervasiveness of patrimonial structures of female subordination is revealed most starkly during the transformation of women's situation that occurs during periods of revolutionary turmoil when those institutions are disintegrating under the force of conflict and the social change that precedes it. Tétreault's chapter delineates these processes theoretically, and the subsequent chapters provide rich empirical detail from 17 different nations.

Some common themes tie the country chapters together. Each documents the often extraordinary but seldom reported levels of participation by women in revolutionary movements. The extent and forms of women's participation vary substantially across nations. In almost all cases, women have performed a variety of support roles that are compatible with the traditional gender-based household division of labor. They provide food and sanctuary for the guerrillas, gather intelligence, and transport supplies and equipment. However, several of the chapters document women's participation in combat roles, including command positions in guerrilla armies. Many revolutionary organizations can point to a handful of extraordinary women who played heroic roles in the movement and assumed leadership positions in the insurgent organization. What is profoundly more significant is the evidence of large numbers of ordinary women serving as anonymous combatants in some of the revolutions. Luta Continua and Catherine Sheldon document this phenomenon in their excellent chapter on Mozambique, as does Sita Ranchod-Nilson in her chapter on Zimbabwe. Both chapters suggest a dynamic by which women can and do assume traditionally male combat roles. First, disruptions in the rural subsistence economy prior to the revolution forces males to leave home in search of wage labor. The resulting breakdown of patrimonial family structures imposes additional burdens on women but also liberates women from many aspects of patrimonial dominance. When they join revolutionary movements, they do so as single women or heads of households, not as spouses of male guerrillas. As such, they are less willing to accept relegation to support roles in the movement.

A second theme is that no matter what the extent of women's participation in the revolution, invariably, the payoffs they receive in the postrevolutionary order are less than their contribution should have merited; and any gains they achieve tend to erode over time. Most newly established revolutionary regimes enact legal and constitutional protections for women's rights. However, constitutional and statutory equality rarely translates into practical equality in the workplace or the political arena. Prerevolutionary patrimonialism creeps back into the workings of postrevolutionary political and economic institutions so that even the limited gains women realize in the immediate aftermath of the revolution begin to erode.

Why this is the case may be a more critical issue than explaining women's participation in revolutionary violence. Tétreault discusses the "dual character" of revolutions as a cause. During the conflict, revolutionary actors cooperate with each other in order to achieve their shared goal of overthrowing an oppressive regime. In this phase, women are promised much because revolutionaries need their support. Once they achieve victory, revolutionary actors compete with each other over the question of who will "control the outcomes of the revolution" and distribute its benefits. At this stage, women are claimants, and the leader's goal is to minimize the number of groups with claims on the fruits of victory. Are women less likely to be excluded in those cases where they have served in combat roles? Kyung Park suggests that they should but notes that the evidence from China and the two Koreas does not support this proposition. Other chapters reach similar conclusions.

Still other chapters suggest that gender interests come into conflict with class interests, dividing rural women against urban women and middle-class women against poor women. Diane Bush and Stephen Mumme's chapter on Mexico draws attention to Maxine Molyneux's distinction between "strategic gender interests" and "practical gender interests" and suggests that the priority that one attaches to these distinct sets of interests varies depending upon one's class status. Thus, poor urban and rural women attach a higher priority to "practical gender interests," such as child nutrition and health, whereas middle-class women may be more concerned with "strategic gender interests," such as eliminating gender discrimination in the workplace or opening up elective office for women candidates. Class-based divergence of interests makes it difficult to mobilize and sustain gender-based collective action that spans class lines, especially in the aftermath of the revolution.

Some chapters are better than others. Kyung Park's chapters on China and the two Koreas are intriguing for their theoretical richness. Connie Jorgensen's chapter on Israel is noteworthy for documenting the reasons for the Israeli regime's failure to deliver on the promise of gender equality. Joan Supplee's chapter on Chile details the failure of the Allendé government to attend to women's issues and the ability of the right to mobilize upper- and middle-class women against Allendé. One could question why some cases were selected while others were excluded. For instance, women participated in combat roles in the Nicaraguan and Salvadoran revolutions, but these cases are not included. However, it is difficult to fault a volume on this basis when it includes 16 chapters on 17 different nations. Tétreault and her collaborators have given us a book that fills a large void in the literature on women and development, as well as women's participation in revolutions.

University of Memphis T. David Mason

Gender and Rurality. Edited by Sarah Whatmore, Terry Marsden, and Philip Lowe. London: Fulton, 1994. 149p. $85.00.

Well into the 1980s, newspapers were full of the devastating effects of the farm crisis in the United States, as overleveraged farmers were forced to declare bankruptcy, sell all or part of their farms, and seek off-farm work. The media offered us images of men and women on the edge of disaster—stalwart women and angry or crestfallen men, some of whom took the route of suicide. Yet it is only recently that social scientists have looked more closely at the diverse effects of that crisis. This slim volume, the sixth in the Critical Perspective on Rural Change Series, explores the interlinkages between gender and rural/urban social geography in advanced industrialized countries—how rurality is shaped by, and may shape, gender relations; the gendered construction of political agency and standing; and feminist theories of science and knowledge as they articulate with alternative modes of environmental thinking. Most of the authors teach or do research in England, Scandinavia, and Australia; and their training is reflected in their close attention to empirical findings, drawn from detailed questionnaires, interviews, and survey data.

The authors interweave multiple levels of analysis in order to get at the "dense, multicolored fabric of rural society" (p. 32). They break apart and question generalizations and permit us insight into the complexity of gender relations. The first set of articles inquires into the gendered nature of rural restructuring in England, Australia, New Zealand, and Canada. Rural restructuring occurs for multiple reasons and therefore has very different consequences, which, in turn, vary for men and women. Where tired urban residents are seeking out a lovely bucolic ideal and are willing to commute to their highly paid urban jobs in order to have that ideal, one finds little male unemployment and many females working in the service sector. In other areas without a proximate urban magnet, there is high unemployment among men and women, and many more women work part-time, rather than full-time, because they believe their primary responsibility should be as mothers for their children. Despite variation in male and female employment, rural women are frequently overeducated for the professions in which they are employed, a situation due to patriarchal gender ideologies.

When rural women become involved in local organizations concerned with social and political agendas connected to the family farm, they have an impact upon the social production of space, that is, "shaping the meaning and representation of rurality" (p. 31). Teather finds two contrasting dynamics among rural women's organizations. In either case, the organizations face an uncertain future. Traditional rural women's organizations, by not taking account of broader social structures affecting the family farm, may contribute to its eventual demise. More progressive organizations are not as popular, because they challenge the status quo and must usually wage a hard struggle to survive. These conclusions support a widely known finding that masculine ideologies tend to become hegemonic, however unconstructive they may be, while alternative ideologies, taking account of women's particular needs and struggles, may foster a common identification among women brought into contact with each other and thereby empower them.

Meyer and Lobao show how state policies structure political activism among men and women. State benefits—social security and unemployment insurance—are regarded as rights for participants in paid work, whereas welfare and food stamps are directed at the household in which participants are considered a "dependent clientele." State policies reinforce existing gender ideologies. Because of the farm crisis and the diversification of farm livelihoods, many women began to work off the farm and consequently were more successful in addressing issues of production and consumption. Meyer and Lobao see women's engagement in paid work, either fulltime or part time, as a potential "basis of future coalition building between progressive segments of the farm and non-farm sectors" (p. 83). A longitudinal study tracking postcrisis political activism among these men and women would be extremely valuable.

A number of authors uncover the more subtle means by which women can—and sometimes do—challenge masculine ideologies about how the family farm should be run, what the division of labor should be, how succession and inheritance should work, and the content of state policies. Carolyn Sachs offers perhaps the most sophisticated interpretation of data and analysis. She traces how women and men are situated with respect to their access to particular rural environments, economic activities, and ownership and control of land. She also traces the evolution of gendered resistance to such problematic hazards as toxic waste sites, arguing against an essentialist portrayal of women as caring, cooperative, and relational. Instead, she stresses that rural women's material reality explains why they are the first to notice and struggle against such hazards as toxic waste in their environment. She is careful to point out that women do not necessarily support environmental issues, because "environmental battles in rural areas are translated into a struggle between jobs versus the environment" (p. 132) and that when they begin to resist the location of hazardous waste facilities in their environment, they do not consider themselves to be feminists. Yet they may experience a transformation in their political perspectives after repeatedly being accused of being "hysterical housewives," having their sanity questioned, fighting against governments and corporations, and realizing that their goals are structurally embedded. They move away from "not in my backyard" arguments to "not in anybody's backyard" stances. Often, they may spearhead resistance to environmental degradation through nonconventional informal means, but men will eventually take over the leadership, leaving behind the broader social agendas that were part of women's struggles. Sachs demonstrates how knowledge claims are "partial and situated," using many of the ideas that Haroway has laid out in uncovering the histories of Western science and technology and how they have been used. Sachs's article contrasts sharply with Schmitt's discussion of ecofeminism and its effects upon German agriculture. Schmitt proposes a utopian scheme for rural agriculture that goes hand in hand with an absolute psychological difference she draws between men and women.

I would have appreciated a summary chapter in which the editors discussed the contributors' findings and placed them in a more general theoretical context, for the conclusions that the latter draw from their material are often contradictory. I was also surprised by the omission of Japan in the volume—a case study of an advanced industrialized society in which the struggles of rice farmers (not examined in depth in terms of gender relations) is well known. There are too many typographical errors and bad translations, given the high cost of the book.

George Mason University LINDA J. SELIGMANN

Economic Change and Political Liberalization in Sub-Saharan Africa. Edited by Jennifer A. Widner. Baltimore: Johns Hopkins University Press, 1994. 307p. $16.95.

The collection is a timely and very lively discussion of the ongoing processes of economic and political restructuring in Africa. The end of the Cold War and the reinvigoration of civil society in the majority of African social formations have encouraged a more critical evaluation of the processes of reform, recovery, and renewal in African states. The initial enthusiasm and euphoria about the processes of political change away from authoritarianism toward multiparty politics has now given way to more nuanced analyses. The linkage between the economic decay, marginalization in an increasingly complex and exploitative global order, rising debt profiles, and failed structural adjustment programs, on the one hand, and internal and external pressures for political liberalization, on the other, is also beginning to receive critical attention. Widner's collection significantly advances the frontiers of knowledge on Africa's experiences at both levels: economic change and political liberalization.

The collection, which contains papers originally presented as discussion papers at a colloquium, "The Economics of Political Liberalization in Africa," held at Harvard University in March 1992, opens with a brief introduction by the editor in which she notes that the reform process in Africa was taking place at a time the world was more favorably disposed to "greater political openness" (p. 1). The introduction tries to link the process of democratization to the traditional preconditions for democratization and democratic consolidation when it doubts the possibility of sustaining the processes of change without "an independent commercial class, elevated literacy rates, and higher levels of per capita income" (p. 2). Perhaps there is no need to examine the issues of liberalization in Africa, because these conditions do not exist. The real issue is that what is defined in the literature as *preconditions* for democracy are exactly the *goals* that people hope to obtain through democracy in developing societies like those of Africa. One does not need a university degree to appreciate and take advantage of guaranteed liberties. Nor is democracy couched only in the languages of the West!

In chapter 1, Robert Bates gives a solid account of the background to reform politics in Africa and contributes to the debate on the linkages between Africa's economic and political crises. He draws attention to the limitations of Africa's reform programs and makes a case for freeing the market to promote initiative and growth as well as the development of a strong bourgeois class. Barbara Grosh examines the place of economic collapse in compelling African states to adopt liberalization as prescribed by the International Monetary Fund and the World Bank. She warns against an overgeneralization of the impacts and implications of adjustment. Her focus on the limits of "populist privatization" is a major contribution that could have been explored further (p. 42). Chapter 3 by the editor compares political reform in Anglophone and Francophone states. Noting that pressures for democratization predated the developments in Eastern Europe, she declares that "domestic pressure triggered the reforms" (p. 49). This chapter includes an excellent discussion of the national conference phenomenon, which has become rather typical of the Francophone nations. Richard Westebbe in chapter 4 examines the case of Benin; Michael Bratton gives a detailed account of the fall of Kenneth Kaunda and the rise of the Movement for Multiparty Democracy in Zambia in chapter 5; and in chapter 6, Nicolas van de Walle examines neopatrimonialism and democracy in Cameroon. In some ways, Walle's use of neopatrimonialism hardly extends our understanding of the real issues behind the actions, power, and weaknesses of African states and their custodians. It fits the traditional stereotype of leaders who simply privatize the state and its resources and feeds into the explanation of the African predicament as arising from irresponsibility and corruption of the elites: Is corruption, patronage, and the abuse of power peculiar to Africa? Why do these leaders find it always necessary to privatize the public sphere and what is the role of imperialism in this tendency? Mwesiga Baregu examines the rise and fall of the one-party state in Tanzania in chapter 7, while Jeffrey Herbst discusses Ghana in chapter 8. They both look at the suffocation of civil society and the obstacles to popular action in the democratization processes.

The third part of the collection examines two major issues: Donald Rothchild discusses state-society relations in which he focuses on varieties of power arrangements and concludes that "effective governance is more encompassing than democracy alone" (p. 224). But can effective governance take place without restructuring the repressive neocolonial state? In fact, "effective governance" and democracy are both less encompassing than *democratization*. Crawford Young in chapter 10 examines the contradictions of a political imperative in Africa's democratization processes. Yet in many places, there is a reification of Africa's problems and steps toward democracy, such as the misplaced praise for Nigeria's Center for Democratic Studies and the National Electoral Commission in the now-aborted zig-zag transition to a third republic. Part 4—the final chapter and epilogue by Ernest J. Wilson II—focuses on "creating a Research Agenda for the Study of Political Change in Africa," with emphasis on the need for a "well-defined focus and a broad-ranging review of the empirical and theoretical materials in order to craft the nuanced theories and explanations" (p. 267). Though skeptical of the overall democratization processes in Africa, Wilson adopts a so-called modified structuralist model, which, like the prodemocracy movements of Africa, fails to pay sufficient attention to the issues of what to do with the repressive neocolonial state that has suffocated civil society for decades; how to contain the influence of power elites who continue to loom large in the transition to democracy; how to design alternative socioeconomic models that will put the masses of Africa in charge of decision making and the democratization agenda; and how to restructure democratization in Africa away from the Western-imposed agenda and the influence of election-monitoring bodies who now have appropriated the right to have the final say on which election is acceptable or not.

Widner's collection, in spite of these observations, is a solid and very powerful contribution to the field. The case study approach provides a deeper excursion into the realities and specificities of African politics. The theoretical and comparative chapters significantly expand our understanding of the complex contours of African politics, in particular the democratization enterprises. The book should stimulate debates for a long time to come.

University of Texas, Austin JULIUS O. IHONVBERE

The Making of Terrorism. By Michel Wieviorka. Chicago: University of Chicago, 1993. 370p. $52.95 cloth, $19.95 paper.

Michel Wieviorka's comparative analysis of Italian, Basque, and Middle Eastern terrorist groups is an impressive and provocative study which makes many valuable contributions to the literature on political violence. In particular, for those of us who have been critical of studies of political violence, especially prominent within the literature on "terrorism", that have relied exclusively on secondary sources, Wieviorka's engagement of actual members of "terrorist" organizations in sociological interventions is a much welcomed methodological innovation. Through these interventions in which terrorist and their potential targets confront each other, Wieviorka elicits, although tenuously at times, important new insights regarding the relationship between terrorists, the movements they claim to represent, the violence they employ, and the social and political institutions they seek to destroy.

Wievorka's study is divided into four parts. In part one, the author sets out the theoretical framework of the book while the remaining sections focus respectively on the Red Brigades and Italian left-wing terrorism, Basque nationalism and the violence of Euskadi ta Askatasuna (ETA), and finally, international Palestinian terrorism.

Wieviorka begins his study by noting that although the theoretical approach he adopts is limited to the analysis of cases of terrorism which "originally arising out of social causes, comes to be transformed into spinoffs linked to leftist ideologies," (p. 4) it should, nonetheless be capable of explaining other forms of terrorism. The central hypothesis guiding Wieviorka's investigation is that terrorism is best understood as the final outcome of a process of inversion by which social movements are first transformed into social antimovements and ultimately into terrorist organizations. A social antimovement, according to Wieviorka, begins by inverting the three fundamental dimensions of social movements: identity, opposition, and totality and culminates in the fusion of their inverted images with the result that terrorism represents the complete negation of the principles and ideals that had inspired their very inception. Terrorists, therefore, Wieviorka maintains, are actors which, "remove themselves from the concrete experience of those in whose name they are fighting, and become heteronomous figures in struggles which are not their own, of which are only theirs in some accessory fashion" (p. 62). The violence which terrorist organizations adopt is similarly explained not as the strategic choice of weak actors confronted by the greater repressive capacity of states, but rather as a substitute for the relationship which the terrorists once, but no longer, shared with those on whose behalf it claims to be fighting. In clearly distinguishing between the method of terrorism and a rationale of terrorism, Wieviorka has clearly mounted a significant challenge to those analysts who characterize all violence by resistance movements as terrorist violence.

In part two, Wieviorka applies his analysis to an examination of west European left-wing terrorism, with a particular focus on the Italian Red Brigades. The author begins with an examination of the concomitant crises within the labor movement and the extraparliamentary left in the post-1972 period during which the Italian Communist Party (PCI) adopted the political line which would eventually lead it to assert its independence vis-a-vis both Moscow and Eurocommunism, and lead it to undertake its historic compromise with the Christian Democrats. As a result of the increasing institutionalization of both the Labor movement and the PCI, Wieviorka asserts that the extraparliamentary Left was torn apart by the tensions between its contradictory desires to constitute a powerful force on the institutional level as well as to serve as the vanguard and mouthpiece for social demands which, in the context of the historical compromise, had no realistic political outlet. The collapse of the extraparliamentary Left, Wieviorka suggests, created the conditions which facilitated the emergence of the Red Brigades by 1) freeing up or strengthening a great number of radicalized collectives whom the armed organizations took it upon themselves to adopt and 2) by opening up an ideological vacuum. The combination of these two dynamics, as illustrated in the conversations among participants in the sociological intervention, Wieviorka asserts, characterize the inversion of the organization, ideology and practice of the Italian radical left and its decent into a terrorism grounded in a reformulation of Marxism-Leninism. This inverted ideology, the author suggests, although it retained a number of its classical canons, stood orthodoxy on its head, and reversed the poles in the relationship between violence and politics. Where the extraparliamentary left had once served as the steward of violence, the author maintains that in its inverted form it is, "no longer able to make a tool of violence, nor can it guide the hand (through the party) of the person who holds the tool, to effect a historical reversal in which the masses must also play a pivotal role" (p. 146). Thus, for Wieviorka, what marks the Red Brigades as a terrorist organizations is not so much its use of extreme forms of violence but the fact that unlike the violence of the labor movement and extraparliamentary left it had deserted each of social reference groups, and had adopted violence as a substitute for those failed social relations. The violence of the Red Brigades was thus a violence bereft of meaning for anyone except the actors.

In part three, Wieviorka turns his attention to an examination of the violent conflict in Basque country. What intrigues Wieviorka, as it has many scholars, is the attempt by ETA and other similar resistance movements to formulate and implement a strategy which is capable, "of freeing in a single movement, a dependent nation and an oppressed social class" (p. 147).

In contrast to his analysis of Italian terrorism, Wieviorka, without explanation, introduces his analysis of violent conflict in the Basque country by problematizing the sociological definition of ETA violence as "terrorist." He begins his investigation by posing two questions that frame his inquiry and the subsequent interventions with the Basque protagonists: 1) Should ETA be regarded as a revolutionary nationalist organization or a terrorist group, and 2) Is it possible to link, through violence, a social focus, a revolutionary vision, and a fight for national liberation, without ultimately sliding into terrorism?

After a brief review of ETA's early organizational history, the author focuses on the organization's development during the early post-Franco period. It was during this initial phase of the democratic transition process that, according to Wieviorka, conditions within Basque society held the potential for triggering the inversion process which could transform ETA from resistance movement to terrorist group. First, the author suggests, the opening up of the political system had made it impossible for ETA to target a single symbolic enemy. Second, Basque nationalism was now interpreted at two levels, the cultural and the political; language could, therefore, no longer play its formerly unifying role as the heart of Basque nationalism. Finally, the social struggles, particularly the labor and antinuclear movements which ETA had championed, had been greatly weakened, thus diminishing, according to the author, ETA's opportunity for maintaining the social relationships necessary to avert the inversion process.

To explain his finding that ETA has not experienced inversion and to defend what he views as the shocking conclusion that "sociologically speaking, ETA violence is, on the whole, not terrorist" (p. 213), Wieviorka attempts to link the popular support which ETA retains in the Basque country to what he refers to as the "myth of the all-Basque movement," (p. 175) i.e., the belief that the struggle is at once national, revolutionary, and social. When, the author suggests, reality appears to correspond to the myth, violence can be held in check and the descent into pure terrorism avoided. It is at this point in his study that Wieviorka's approach appears most strained and the strategic approach which he earlier rejects as inadequate reappears to offer a more plausible explanation for the continued violence in the Basque country. Wieviorka, himself, suggests this by noting that, "in the present situation, . . . military or police intervention cannot hope, through independent action, to wipe out the armed struggle, whose popular support base remains massive" (p. 214).

In the final part of his study, Wieviorka applies his theory of sociological inversion to an analysis of international terrorism with a focus on Palestinian and Lebanese actors. In particular, the author contrasts his approach with those which explain terrorism from a purely instrumental perspective, which focus on the internal dynamics of the decision-making processes as potential triggers for terrorism, and those which refuse to acknowledge any differentiation between the Palestinian movement, its organizations, and the practice of terrorism.

After a detailed review of the organizational history of the Palestinian movement in which the author focuses on the trajectories of the various political organizations and processes of its constituent parts, Wieviorka comes to the not so surprising conclusion that it is difficult to, "distill the Palestinian practice of terrorism down to a single essence or to quality the Palestinian movement, once and for all, as a terrorist movement" (p. 259). Rather, he argues, that his research reveals the often contradictory nature of terrorism which can, at times, "constitute a method to be used as a particular juncture (Fatah, Munich 1972)" as well as its own rationale of actions when it "becomes the expression of a veritable inversion" (p. 260). Wieviorka's examination of the Lebanese actors, including Hizbollah and the Amal movement, leads to a similar conclusion.

In essence, Wieviorka argues, the dynamics of terrorism, whether they operate within a single state or across multiple states, remain the same: to the extent that the actors employing the violence remain connected both socially and ideologically to those in whose name they are fighting one can speak of political violence but not of terrorism which implies the radical disengagement by the terrorist actor from the social, national or communal movement in whose name he or she acts.

Without doubt, this study represents a major intervention in the literature on political violence. It is extremely rich in theory and fact, and provocative in its method and conclusion. Its only weakness lies in the often tenuous relationship between the evidence drawn from the sociological interventions and the author's rejection of possible alternative explanations for resistance movements' adoption and use of "terrorist" violence. Wieviorka's contributions to the work in the field, will, nonetheless, be lasting and significant.

University of Kentucky CYNTHIA L. IRVIN

INTERNATIONAL RELATIONS

Political Institutions and Military Change: Lessons from Peripheral Wars. By Deborah D. Avant. Ithaca: Cornell University Press, 1994. 161p. $29.95.

Deborah Avant's book provides a creative and useful application of institutional political economy to a new field—the development of military doctrine in democratic states. Her well-argued first and second chapters provide a host of examples of how concepts commonly used by those studying the behavior of firms and legislatures can also be applied to the issue of innovation in democratic military organizations. She casts military officers as agents of political leaders, who are themselves agents of the electorate; portrays the military's propensity to innovate as a function of the structure of employment incentives that officers face and writes of the ensuing strategic choices that officers make; and describes the moral hazard confronting political leaders when they rely on specialized information controlled by military officers. To my knowledge, her effort to apply this perspective to civil-military relations is unprecedented, making this a truly pathbreaking book.

Avant uses this perspective to explain why British forces did a better job of integrating military doctrine with political grand strategy, in both the Boer War and the Malayan Emergency, than did the American forces in the Vietnam conflict. The American picture she paints is of a dominant army wedded to concepts appropriate to the European theater and able to dance around presidential attempts to instill counterinsurgency plans by focusing on a Congress sympathetic to traditional large-offensive plans. The British picture is of a military whose leading officers had been appointed because of their ability to react flexibly to a changing environment and controlled by a unified cabinet unhampered by checks and balances. She thus argues that the differing structure of legislative-executive relations in parliamentary and presidential systems is the primary explanation for the differing level of integration between doctrine and grand strategy in the two countries.

The empirical evidence she presents bears out her argument that military officers react strategically to their institutional environments. Particularly strong is her contention that the U.S. president can use the ability to promote and fire officers only as long as congressional approval for such actions is forthcoming and that the army has thus been able to appeal to Congress when it wants to circumvent presidential oversight. It is clear that the structure of the political system matters in determining how well civilians can control military operations.

The aspect of Avant's story that is less satisfying is the simple fact that the doctrines and strategies chosen by civilian and military leaders have depended on unique juxtapositions of historical events. Domestic political structures have created conditions that favor the implementation of certain policies over others, but those structures did not cause actors to develop the preferences they did and thus did not determine outcomes. For example, according to Avant, one of the primary reasons that the U.S. Army entered the Cold War era with a focus on large-scale offensives and on unity of perspectives across the officer corps was its nineteenth century grounding in the Prussian ideal. This coincided, she argues, with a congressional budgetary focus on the possibility of large-scale world war against the Soviet Union. She shows that at the end of the nineteenth-century, Congress gave the army the autonomy to pursue its own training and promotion policies and that this is what allowed the army to focus on Prussia. But nothing dictated that the army would choose a model that would arguably turn out to be so inappropriate for the second half of the twentieth century. We can imagine a counterfactual situation, where both the Civil War and the resultant professionalization of the U.S. Army would be delayed by a decade or two. In that hypothetical circumstance, army leaders might have taken as their founding principle the lessons of flexibility taught by the Boer War, rather than the rigid planning of the Prussians. Certainly, domestic political structures would still have played a role in shaping military policy and the relationship between grand strategy and doctrine. Yet the fact of checks and balances in the American political system would not, in and of itself, have necessarily caused the U.S. Army to favor an inflexible policy opposed to the preferences of the president (a possibility Avant recognizes on p. 34). Had historical circumstances been different, the same system could have found a president and an army sharing a vision of what operational necessity entailed. Officers would still have acted strategically to maximize the army budget, and executive-congressional relations would still have been important factors in explaining outcomes; but we might have seen an army doctrine better integrated with presidential views of national strategy.

Counterfactual analysis could also lead us to postulate a different outcome for the British. (For example, if officers in the mid nineteenth century had not been expected to be gentlemen first, a unified military perspective could have developed that would have been opposed to attempts at intervention by unified civilians.) Avant is clearly aware of how dependent her argument is on historical accident. Throughout, she talks about the unique "biases" of each of the organizations she studies. Her concluding chapter states that "this model makes no absolute predictions free from historical context" (p. 138). The explanatory nature of her model is, by necessity, post hoc; and its precepts of agency, strategic behavior, and moral hazard can, in fact, be used to explain virtually any civil-military outcome after the fact.

Avant nonetheless tells an interesting set of stories from a unique perspective. She has made an important contribution to the development of civil-military relations theory, proving through her careful analysis that it makes sense to integrate the new institutionalism into the subfield, particularly by focusing on budgetary and hiring procedures.

Ohio State University Kimberly Marten Zisk

Women, Gender, and World Politics: Perspectives, Policies, and Prospects. Edited by Peter R. Beckman and Francine D'Amico. Westport, CT: Bergin & Garvey, 1994. 248p. $59.95 cloth, $19.95 paper.

Beckman and D'Amico describe the book as "an on-going student-teacher dialogue" and invite their readers to participate in exploring the connections among women, gender, and world politics (p. xi). They effectively promote such participation through a well conceived organizational format, clearly written and very informative chapters, and close editing that ensures prose consistency and draws our attention to linkages throughout the text. Each essay deals concisely, coherently, and at a similar level of sophistication with its assigned topic. Designed as an introductory textbook, this collection succeeds admirably in being user-friendly and drawing the reader in.

In a succinct introduction, the editors distinguish biological sex from culturally constructed gender and *gender-as-difference* (differences between men/masculinity and women/femininity) from *gender-as-power* ("a *relationship* between humans . . . that is based on power [p. 6]). From the latter perspective, women, their experiences, and feminine characteristics are devalued as a result of men's power over women and the division of public (political) and private (apolitical) exemplifies how cultural definitions of gender render "certain things politically invisible, and women . . . invisible as political beings" (p. 7).

Part 1, "Perspectives" responds to the question, How do we think about world politics? Each author sets out the assumptions and argumentation of a particular theory and examines these by reference to three central questions: (1) how the theory deals with women and addresses gender, (2) what a gender-sensitive critique might include, and (3) how gender-conscious theories improve our understanding of world politics. (A companion volume, *Women in World Politics*, explores the question, How have women been *participants* in world politics?)

Beckman's review of realism and neorealism concludes that their preoccupation with states, anarchy, self-help and balance of power appears to render gender irrelevant: "In an anarchic world, states led by females will be subject to the same forces as the current predominantly male-led states" (p. 22). J. Ann Tickner's feminist critique exposes pervasive yet unacknowledged androcentrism (male-as-norm assumptions) in realist constructions of the individual ("political man"), the state (masculine), and the international system ("war of everyman against everyman"). Karen Feste reviews "the scientific approach" and major behavioralist projects, confirms that women have indeed been excluded, and suggests how inclusion might be operationalized. D'Amico notes that pluralist and critical perspectives offer alternatives to realism and promise more inclusive explanations (p. 55) but concludes that they fail to take gender-as-power seriously. At best, a few pluralists consider adding women, and critical theorists add them primarily as workers.

Turning to feminist perspectives, Sandra Whitworth critically reviews liberal feminism, which brings women in radical feminism, which depicts woman as nurturer, and feminist postmodernism, which deconstructs 'woman'. Her preference is a critical/feminist perspective that incorporates the insights of these approaches while overcoming their limitations (p. 83). Addressing Third World feminist perspectives, Hamideh Sedghi considers the experience and perceptions of women in four struggles (Turkish nationalism, Algerian national liberation, the Iran-Iraq regional war, and the Persian Gulf War). This important chapter exposes the dangers and effects of (Eurocentric, feminist, nationalist) homogenization; the error of single-sided and/or monocausal explanations; and the need for complex, historically situated perspectives.

In Part 2, "Policies," Jean Bethke Elshtain considers how war narratives, in general, produce gendered identities and activities. Bringing the local and global into relation, Rebecca Grant explores how the feminine mystique that circumscribed women's lives in the United States was shaped by the Cold War. Margaret Galey provides a concise overview of how the United Nation addresses women's issues and affects women's lives. Turning to economic issues, Nuket Kardam contextualizes and summarizes development assistance policies and evaluates how they shape women's lives and possibilities for empowerment. And in a key chapter, Geeta Chowdhry reviews and critically assesses theories of International Political Economy from a variety of feminist perspectives.

Part 3, "Prospects," considers alternative futures and what is required for their realization. Birgit Brock-Utne focuses on adult education as a hospitable environment for peace education but insists that the latter will not produce peace if it does not address gender. Reviewing a variety of feminist perspectives, Ralph Pettman considers what each offers in the way of alternative futures. In a fascinating chapter, Anne Sisson Runyan assesses the contribution of radical feminism by considering how feminist fictional utopias "allow us to imagine, however partially and imperfectly, ways in which we might rethink personal, societal, and intersocietal relations" (p. 214).

In a concluding essay, the editors provide excellent resources for pursuing the question, "What next?" They specify a framework for asking how gender affects world politics (and vice versa), suggest gender-sensitive questions to ask of other texts, and identify additional literature on the study of women and gender in world politics. In an extremely helpful discussion of types of questions (i.e., *making-connections*, *under-what-conditions*, *change-across-time*, *unintended-consequences*, *context*, and *what-else*), the editors demonstrate both how these questions were put to work in the volume and how the reader might employ them in related or innovative ways.

This accessible, engaging, and very instructive text enables us to see how men and women are affected differently by international relations and how adding women and femininity might alter the study and practice of world politics. As an advocate of feminist theory/practice in international relations, I welcome this pedagogically successful contribution. As someone attentive to theoretical debates, I am critical of how the text deploys *gender*. Specifically, the volume deals almost exclusively with gender as what we conventionally think of as *sex*-differentiated identities (masculine-feminine), roles (soldier-mother) and activities (public-private). Demonstrating that gender is about men and masculinity (because these are *constituted* by their polar opposition to women and femininity) is a singularly important contribution to a discipline that continues to associate gender exclusively with women. But a focus on gender as masculine *or* feminine obscures the systemic and transformative implications of gender as an analytic category that shapes not only who we are and what we do but how we conceptualize and claim to know. The androcentrism of binary thought per se is never effectively exposed, much less contested. Neglecting gender as an analytic category, the volume fails to delink gender from ostensibly natural sex differences and, in effect, reproduces the dichotomy of sex, in particular, and the discourse of binary logic, in general. It leaves the power relations sustained by either/or (binary) thinking in place. In short, gender-as-differences so dominates the text that gender-as-power, empirically *and* conceptually, is rendered invisible. The text remains an important introductory contribution, but its construction of gender must be treated with caution.

University of Arizona V. Spike Peterson

Confidence Building Measures in the Middle East. Edited by Gabriel Ben-Dor and David B. Dewitt. Boulder: Westview, 1994. 389p. $59.95.

The editors have assembled a series of studies aimed at assessing the extent to which *confidence-building measures* (CBMs)—first developed as such during the Cold War in East-West relations—may prove helpful in reducing tensions and resolving deep-seated conflicts in the very different setting of the Middle East. One is tempted to conclude (as do several of the specialists whose work is represented in this volume) that the problem is largely tautological in nature: the structural and substantive discrepancies between Europe during the détente period and the Middle East since partition (and before) mean that CBMs (i.e., steps affecting the disposition of military forces so as to reduce the chances that miscalculations could lead to war) cannot be effectively applied in the latter. In effect, the conditions needed to make CBMs effective (mutual acceptance of the legitimacy of all parties to the Arab-Israeli conflict) also render CBMs less important, as a derivative element of political conflict and conflict resolution.

In fact, the merit of this work is that it shows just how difficult it will be to elaborate a system of CBMs in the Middle East *and* how helpful CBMs have been in specific circumstances, especially Egyptian-Israeli relations before the Camp David accords of 1979. The reader is repeatedly—and justly—cautioned about applying the East-West experience with CBMs to the Middle East without a detailed consideration of local circumstances. East-West CBMs were focused on states, on militaries, and on stabilizing a status quo that both sides were evidently unwilling to challenge at the risk of direct confrontation, much less war. In the Middle East, by contrast, effective CBMs will have to be convincing to broad publics, not just governmental elites; address issues of intracommunal violence as much as, if not more than, questions of military deployments; and be developed in a setting in which recognition of the other (a precondition for the establishment of confidence, whether between states, societies, or individuals) is itself either in question or an essential trump in negotiations on peace and security; that is, what is process for one is often substance for the other.

Moreover, the fault lines of tension in the Middle East run across and within Arab states and societies, quite apart from the Arab-Israeli conflict, thereby rendering the application of CBMs developed in the bipolar world of Cold War Europe still more problematic. Yet something has been happening (in Egyptian-Israeli relations for two decades now) suggesting that the techniques of building confidence retain a qualified importance for the region. The trajectory of Israeli relations with the Palestinians, Jordan, and Syria (quite apart from Egypt) indicates that the most important condition for building confidence now exists, that is, recognition of the importance of a relationship qua relationship and a mutual willingness to invest in it, even separately from the issues involved.

Janice Gross Stein notes in this respect that CBMs have played a critical role at every stage in the negotiation of security arrangements between Israel and Egypt (pp. 199–217). Such measures as demilitarized zones, the interposition of international observer forces, establishment of direct communications, and joint consultative commissions to resolve disputes over the interpretation of data have helped to resolve Egyptian and Israeli dilemmas of cooperation. At a minimum, the presence of a United Nation force on the Golan Heights "underscores the reality that neither side is seriously considering a military challenge in the near future, despite the continuing high level of instability" (p. 72).

Still, most of the authors seem to agree that a broader peace in the Middle East will create as many threats (and thus crises of confidence) to particular governments, movements, and societies as it will remove. If it is true that the "only way to veritably reduce hostility in the Middle East would be some fundamental changes in Arab-Muslim political culture" (as Gideon Gera argues, p. 162), then CBMs cannot be seen as a framework for enhancing stability throughout the region but, rather, as marginally helpful in highly specific situations and circumstances.

The annotated bibliography is an excellent guide to the literature on CBMs, in concept and in application to the East-West conflict, the Middle East, and other regions. In addition, the analysis of the difficulties in applying the CBM concept and experience from East-West relations to the Middle East suggests just how much the parties to the Cold War actually agreed on during the final decades of their fateful coexistence.

University of Virginia Allen Lynch

Crises in World Politics: Theory & Reality. By Michael Brecher. New York: Pergamon, 1993. 676p. $112.00 cloth, $48.00 paper.

Between 1918 and 1988 various national governments of the world experienced 826 foreign policy crises that aggregated into 390 international crises. They were enormously varied episodes. Can they reveal any patterns about the conditions under which crises escalate and become violent? That question drives this ambitious work. To conduct the inquiry, Brecher constructs and tests separate models for crises at both the state and the international-system levels and then merges them into a unified model. The result is a general model designed to explain twentieth-century crises that involved a risk of war or expansion of an existing violent conflict. To investigate the model, Brecher advances a number of hypotheses that are explored with the aggregate data on all the identified crises between 1918 and 1988, plus 11 case studies including the 1990–91 Gulf Crisis, which receives an extended treatment.

For Brecher, a foreign policy crisis arises for a state when its policymakers perceive three harmful conditions: threat to important values, time pressure, and a heightened sense of the likelihood of war. When two or more adversaries are in such a condition and engage in disruptive interaction, the author stipulates the existence of an international crisis. The foreign policy model consists of four stages or periods (precrisis, crisis, endcrisis and postcrisis) paralleling four phases in the international model (onset, escalation, deescalation, and impact). Movement from one stage to another is conceived as a step-level change dependent on a trigger in the form of an act, event, or environmental change that alters (i.e., increases or decreases) an actor's perception of threat, time, or likelihood of war. Brecher constructs hypotheses about systemic, interactor, actor, and situational attributes that affect perceptions of threat and the associated elements of harm.

The crisis hypotheses are like those given the public suggesting that the likelihood of heart problems can be reduced by exercise, proper eating, nonsmoking, and the selection of "genetically correct" parents. How much each component contributes to reduce risk and whether they work in a particular case is uncertain. So, too, with the crisis hypotheses. Thus, a state is most likely to escalate another's crisis when it is ruled by a new, nondemocratic regime experiencing internal instability, is part of a protracted conflict, and is geographically proximate to its adversary. Although the logic for each separate attribute's effect on crisis is examined, how they might interact together is not. (The author acknowledges that some independent variables impinge on others, but nothing approximating sequential causal modeling is advanced either verbally or statistically.) Some hypotheses are constructed by augmenting the conditions specified for a prior hypothesis with additional variables but with minimal explanation for the specific effect contributed by the supplemental variables. For example, without elaboration, the crisis escalation just mentioned is postulated to more likely be violent if the escalating state is receiving military support from a major power (p. 174). It is not difficult to speculate why this might be so; but it is less obvious why military support (the supplemental variable) would be the logical additive for violent escalation, rather than a substantial power differential between the adversaries (one of the variables specified as contributing to any escalation.) The point is that the logic for including some variables at one point rather than another is unclear. Readers also may find a few hypotheses bordering on tautologies, as when a crisis is more likely to escalate to severe violence when the triggering act is violent and the response is equally severe (p. 157).

The conceptualization of crises at two levels—international and state—is one of the many innovative features of this volume. Brecher seeks the explanation of international crises at the state level. More specifically, he attributes the dynamics of crises to the actions of policymakers understood as resulting from their perception of harm, which creates stress, which affects the coping behavior of the policymakers. Exactly how crisis-induced stress affects behavior is one of the puzzles of this book. Brecher approvingly quotes Lazarus and Folkman's *Stress, Appraisal, and Coping* (1984), which defines individual stress "as taxing or exceeding his or her resources and endangering his or her well-being" (p. 45). Throughout his book, Brecher carefully reviews the coping of decision makers in each of 11 cases of crisis. In the final chapter, he finds, based on this evidence, a much more mixed impression about the effects of stress on crisis management (i.e., both positive and negative features). This conclusion would suggest that a theory based upon the effects of stress would need to differentiate with considerable precision when the negative consequences will prevail and when they will not.

For multiple reasons, Brecher's book will be an essential touchstone for the study of crises for years to come. It provides a comprehensive review of much relevant scholarship and case material. It offers an impressive interplay of insights from both crisis case studies and findings presented as descriptive statistics from a major aggregate data set. It advances an inventive multilevel and multistage approach that includes the often-neglected deescalation and impact phases; and the latter stage makes the case for why crises can be critical for both states and the international system. Finally, it presents a number of findings, including puzzles and unconfirmed expectations, that should stimulate considerably more research.

Ohio State University CHARLES F. HERMANN

Global Accord: Environmental Challenges and International Responses. Edited by Nazli Choucri. Cambridge: MIT Press, 1993. 562p. $50.00.

The great increase in international political activity on environmental issues over the past 25 years has spawned two distinctly different responses among social scientists. One response, locating itself under the rubric of "global change," seeks to incorporate physical and biological processes into political and economic models. The core idea is that what humans do to the environment—and, in turn, what pressure environmental change exerts on humans—can be treated endogenously alongside political and economic phenomena. This approach tends to be pursued by geographers and by analysts trained originally in the physical sciences. An entirely different response has been to apply theories of international relations to environmental issues, modifying such theories as necessary but not going so far as to treat the environment endogenously in an integrated model. This route is pursued more commonly by those trained in conventional international relations theory.

Choucri intends this book to contribute to the first task of building a fully endogenous model. It aims to develop "an integrated conceptual framework" linking three fundamental processes (political, economic, and environmental) operating at four levels of analysis (individual, national, international, and global) (p. 3). Such a framework is needed, Choucri argues, if we are adequately to understand and respond to such global phenomena as climate change.

Part 1 approaches the task with four chapters, each of which directs its gaze to the entire topic of global change. Part 2 then examines specific actors and processes, such as multinational corporations (Choucri), environmental management (Garry Brewer), and science and technology (Eugene Skolnikoff). Part 3 focuses on economics and the law, including contributions from Jerome Rothenberg and Edith Brown Weiss. In part 4, Oran Young, Peter Haas, and others explore international institutions. And finally, in part 5, Choucri and Robert North draw everything together in a synthesis. Altogether, the book spans 15 chapters.

The goal of an integrated conceptual framework proves elusive. Most chapters read as stand-alone pieces of analysis that speak to each other very little; it is hard to see the presence of the opening analytical framework in any of the interior chapters. Virtually the only integration that takes place in the book occurs in the first and last chapters, which go little deeper than pointing out the ecological truism that everything is connected to everything else. To be fair, it is hard to imagine how any effort to integrate such complex phenomena could ever succeed. (Imagine trying to be Talcott Parsons, John Maynard Keynes, and Louis Agassiz all at the same time and getting everything—indeed, anything—right.) Therefore, the more realistic standard for judging this book is how well the individual chapters contribute to specific strands of our broader effort to understand global change.

Approximately two-thirds or so of the book reports on the state of scholarship that was ongoing before this book was conceived. Many of these chapters are quite good; but, given the lack of integration, any impact their ideas have will come largely from their authors' larger research programs, not from their inclusion in this volume. Among the more interesting novel contributions are Choucri and North's creative attempt to apply an updated version of lateral pressure theory to phenomena of global change (chap. 3), Alker and Haas's artful and sweeping discussion of the evolution of environmentalism as a social idea (chap. 4), and Haas and Sundgren's effort to quantify some of the more interesting trends in international environmental cooperation (chap. 12). These three chapters constitute important contributions to the broader research program of understanding global change.

Choucri and North's chapter is important for providing one of the few models of a social science theory that treats physical and biological processes endogenously. Its major weakness is that like the overall volume, it incorporates so many disparate phenomena into a single analytical model that for most social scientists, it will appear unwieldy. Still, the model is articulated clearly enough that it ought to permit rigorous testing of narrow pieces of it.

Alker and Haas's chapter is valuable for tracing, in a sophisticated way, the interplay between ideas in the scientific and social realms and for grounding such exploration in a historical perspective far deeper than is commonly employed. It tantalizes with a number of interesting propositions worthy of further exploration. It will disappoint those who wish to see more investigation into the independent influence of ideas on practice. Such an effort is not precluded by the analysis offered here, however. In fact, anyone wishing to make such effort will be significantly aided by a close reading of this chapter.

The chapter by Haas with Sundgren is based on a data base of 132 environmental treaties signed between 1921 and 1989. The great value of this chapter is simply that it shows that quantitative analysis of international environmental politics is both possible and desirable. The chapter identifies some interesting trends concerning treaties' scope, their intrusiveness into state sovereignty, and the role of hegemony. As the first effort of its kind, it is not without its rough edges. Some weaknesses are a function of the raw material available for quantification. (For example, the variable *effectiveness* is blank for 118 of the 132 treaties, solely because there are so few studies of effectiveness to draw on.) Other problems could have been avoided. For example, the analysis draws conclusions about the relative difficulty of solving collaboration-versus-coordination games, even though the universe of cases examined includes only successes. These weaknesses are outweighed by the chapter's strengths, and the effort is highly worthy of follow-up and emulation.

This is a big book about a big topic. Although few readers will benefit from reading it from cover to cover, each chapter has something important to offer. Many of the chapters reflect some of the best ongoing work on questions of global change. Some constitute important original advances in the burgeoning research program. These features merit the book's space on library shelves and course reading lists.

Princeton University MARC A. LEVY

The New European Security Disorder. By Simon Duke. New York: St. Martin's, 1994. 448p. $49.95.

Simon Duke has written a paradoxical book unlikely to be read by those who would surely benefit greatly from it. It is a long, detailed, and generally well argued book on, essentially, policy questions. European security would definitely benefit if all policymakers were forced to read this book immediately. But this most likely will not be. How many decision makers read a book of 400 pages?

It is an impressive feat to bring all the different chapters up to date simultaneously and to cover all main actors, issues, and institutions in European security; but because the book is almost completely without theory, there is a high risk that it will date rather soon.

The overall structure of the book is (1) threats, (2) main actors, (3) institutions, and (4) responses so far to the problems of the "East Europeans." It leads to some clear recommendations: most controversially, he argues a phasing out of NATO in favor of a pan-European security structure.

One of the book's main underlying ideas is that Cold War logic lingers on and that this prevents some of the new thinking necessary to handle a changed situation. Duke has an interesting theme here, which he might have carried through more forcefully to the concluding discussions. As it stands, the thesis is not really argued, though it probably could be.

Inspired by this overall idea of unqualified continuation of old thinking, the book has many good warnings against easy interpretations of new events in terms of old schemata. For instance, it is pointed out that *hypernationalism* is a phenomenon among the smaller powers, not among the main powers, a main difference from the oft-evoked interwar period (pp. 22, 67). These and other observations help to counter the generalized pessimism and self-fulfilling prophesies widespread today.

Chapter 1 is devoted to dispelling two exaggerated threats: an interpretation of the present time as akin to the interwar years or other unpleasant periods and the perception of Islam as a threat. Chapter 2 then presents the threats, Duke wants us to take weapons proliferation (esp. in the ex-Soviet area, nationalism and irredentism, migration, Germany, and something about the global threats seriously. The selection is neither obvious nor really argued, but it gives quite a good sense of the diversity of problems.

Chapters 3–5 look at the main powers and nicely argue the case for a "reverse-realist paradigm" in relation to leadership (pp. 7, 94): where the major powers should be expected to contend for leadership, they almost struggle to avoid it: France sees itself as leader, but it is not clear how this could be realized; Britain consistently marginalizes itself; Germany has various domestic blockings; and the United States is one of the main causes for the lack of leadership in Europe (pp. 114, 130, 134, 164).

Chapter 5 is a good discussion of the U.S. role in European security. It is more polemical than the other chapters. Against the "almost paranoid devotion to NATO," it advocates a policy of transition from a trans-Atlantic security system to a more European one, even to the point of dismantling NATO in order to preserve American influence, instead of risking it all in stubborn conservatism (p. 173).

Chapter 6 on "security architecture" contains an intersting argument against overemphasizing institutions—which is useful in producing a more cautious and self-conscious discussion in the following chapters on the various institutions. They are not treated as solutions per se but as, at best, facilitating cooperation, stabilization, and conflict management. Chapters 7–9 are historical narratives of the European Union (EU)/Western European Union (WEU), the Conference on Security and Cooperation in Europe (CSCE), (now Organization on Security and Cooperation in Europe, OSCE) and NATO, and will undoubtedly be useful to many students.

One can see in the WEU and NATO chapters how the book begins to date already, by not integrating the launch, at the January 1994 NATO summit, of the Combined Joint Task Forces (CJTF) concept, which might indicate possible pragmatic ways to facilitate synergy between NATO and the construction of a European "defence identity."

Generally, the book takes a rather traditional view of Eurosecurity as competing with the United States/NATO. This might stem from a too anti-American interpretation of French policy. In the nice formula of Ulla Holm, French policy during the Cold War strove to keep the Americans in militarily and out politically, while the Russians should be in politically, but kept out militarily; that is, one wanted American troops but not American dominance of European affairs, and the Russian tanks should, of course, be kept out, but France needed Russia as political player. This formula has actually changed little by the end of the Cold War, except that there is now less reason to fear American dominance. Thus, a more relaxed French attitude to NATO and the United States is partly explained by French marginalization but also by the fact that France can safely reengage because the United States is no longer a threat. This can possibly lead to a de facto Paris-Washington axis (briefly touched upon on p. 115); it is between Washington and Paris that all major deals have to be struck: when they agree, all agree.

The three last chapters discuss the attempts and limitations to pan-European security: the Stability Pact, economic assistance, and the Bosnian "crisis," respectively.

The book is atheoretical in two ways: its concept of security and its international relations (or security) theory. The declaratory statements on a wider concept of security stands in contrasts to an empirical analysis that converges around defence policies. For instance, half of chapter 2 is spent on a (valuable) discussion of ex-Soviet nuclear weapons. Because there is no explicit reflection on the concept of security, the book takes the typical not-only-but-also stance where security is military-linked and militarylike—only a bit wider. Implicitly, military questions are at the center, around which other questions circulate. To get away from this ironic result, it is necessary to have a clear concept of security, implying that one can specify when military problems are indeed the most important security issues but also when military issues are secondary to other types of threats.

In the competent discussion of French security policy, the interest in a European security option is explained from a relatively narrow discussion of the defence policies of various countries (the United States and Germany), but the wider concepts of what part of Europe France wants and the place of European *integration* are ignored. There is a consistent emphasis on military issues, such as force restructuring (esp. nuclear weapons), and a consistent underestimation of integration as a security system (probably Europe's most important nonmilitary security). Through a process of integration, political-economic forces reshape continental dynamics. But the EU is discussed almost exclusively in relations to its possible defence role. A paradoxical value of the book in relation to its declared purpose is that it relocates questions of nuclear weapons, commonly forgotten these days.

In the Germany chapter, Duke discusses restraint on the use of military force for out-of-area purposes and much less space on the factors claimed to explain German foreign policy change compared to previous Germanies, namely, economic interdependence and the fact that Germany itself (its mentality and political structure) has changed. In relation to ex-Yugoslavia, there is an extensive discussion of Germany's qualms over military participation but not a similarly extensive treatment of the German recognition of Croatia and Slovenia, seen by most other states as the most disturbing instance of German unilateralism and assertiveness. Thus, the interpretation of this event is in itself a major factor in current security affairs.

The most striking illustration is the section on institutions. The EU chapter only deals with the common foreign and security policy, the WEU, previous attempts to form defence cooperation, and the Euro-Corps. Nonmilitary-security functions of the EU are not discussed. Somewhat conventionally, it is assumed that "conflict prevention" will have to be done by the CSCE (and be declared as such). More important is probably the implicit, unofficial stabilizing functions of European integration. One could even claim that there is a logical sequence of institutions: the last to arrive, when conflict has broken out, are NATO and (potentially) the WEU. Before that, trying to mediate in conflicts threatening to escalate, we find the OSCE; but even before that, the EU has the role to change motivation and orientation, for example, Hungary as it orientates itself toward acceptance in the EU and therefore downplays grievances over minorities in relation to neighboring countries.

Some conflicts do not even appear as conflicts—the parties do not even get to articulate each other as opponents—which testifies to the general shaping powers of a process of integration, patterning Europe around one center in contrast to its multicentric balance of power in the past.

In relation to theory and method, Duke argues why none of the big theories work (pp. 9–10). That is okay, but what, then, about quasi theories about different kinds of security systems—collective security, alliances, integration? This literature is given no systematic treatment. The most important effect is that the case made for the CSCE as a pan-European security institution remains weak. The recommendation mainly follows from the fact that the problems are all-European but does not systematically address the question whether collective security or other types of security systems are likely to be the best solutions.

Despite the lack of theory, Duke makes constant (negative) references to neorealism. It seems to be a bad habit in many current books to draw theoretical conclusions from atheoretical discussions combined with superficial presentations of the theories to be bashed. Even more surprisingly, Duke's own analysis is still mainly in terms of states and (their) institutions, rational calculations of interests, and so on. This is a break with realism only if one accepts the (admittedly widespread) strawman image of (neo-)realism as, for example, insisting that only structure counts, that no domestic or transnational factors can be of relevance, and that power is necessarily military—and other strange misreadings. Not that I would want Duke necessarily to (re)turn to neorealism. His declared intention to break away from neorealism is hampered by rather unclear ideas of the theoretical issues at stake.

The many interesting tables, maps, and opinion data make it a useful book for students—to be used together with more theoretical articles, for example. Duke's book supplies much of the empirical knowledge that American students in particular may need in order to take an independent stand in relation to the theory debates and test different theories on the case of European security. Despite Duke's many remarks on failures of neorealism, the book in itself cannot be said to have contributed much explicitly to the field of theory and European security. The ultimate paradox is that Simon Duke has written the best book presently available regarding the place and role of military matters in the new European security disorder.

Centre for Peace and Conflict Research OLE WAEVER

The Political Economy of European Monetary Unification. Edited by Barry Eichengreen and Jeffrey Frieden. Boulder: Westview, 1994. 198p. $52.50.

The Maastricht Treaty, which entered into force on 1 November 1993 and is subject to a review in 1996, established a European Union consisting of three parts, or *pillars*. Pillar 1 amended the Treaties of Rome and Paris concerning the original European Community. It provided a timetable for completion of European monetary unification, renamed the European Economic Community the European Community, and supplemented the statutory powers of the Community in certain areas. Pillar 2 concerned the common foreign and security policy, and pillar 3 covered justice and internal affairs. *European Union* is now the umbrella term referring to these three pillars.

The Political Economy of European Monetary Unification is a collection of essays produced by a study group on the political economy of European integration, convened by the editors on behalf of the Center for German and European Studies of the University of California and funded by the Federal Republic of Germany. The nine essays deal with pillar 1 of the Maastricht Treaty—the creation of a single (not a "common") European currency and the establishment of a European central bank (the Eurofed).

The individual essays present a wide range of quality, readability, and usefulness. Some are quite good and are recommended on their own merits; others appear to have been included because of the authors' participation in the study group and not for the content of the individual contribution.

The introductory essay, "The Political Economy of European Monetary Unification," by Eichengreen and Frieden, is the best of the nine chapters. This chapter rightly argues that European monetary unification is both an economic and a political phenomenon and presents an extended review of the relevant literature. The authors conclude that European monetary unification is driven mainly by political, rather than economic, factors, a conclusion that most observers have been advancing for some time.

A second informative chapter is Benjamin J. Cohen's "Beyond EMU: The Problem of Sustainability." Cohen challenges the assumption that monetary union is irreversible. This chapter contrasts the experience of three surviving monetary unions (the Belgium-Luxembourg Union, the CFA Franc Zone, and the East Caribbean Currency Area) with three that were not successful (the East African Community, the Latin Union, and the Scandinavian Union). Cohen concludes that while some economic/structural factors (e.g., high factor mobility, an independent central bank) are necessary, the presence of a political cohesion (or a community) is needed to maintain a monetary union over time.

Two of the more specialized entries are "On the Feasibility of a One-Speed or Multi-Speed European Monetary Union," by Alberto Alesina and Vittorio Grilli, and "The Transition to European Monetary Union and the European Monetary Institute," by Jürgen von Hagen and Michele Fratianni. Employing econometric modeling techniques, these two essays conclude (1) that a multispeed approach is less feasible than a one-speed process and (2) that the European Monetary Institute is an inefficient solution, due to its being dominated by national central bankers and by the conflict within the European Union between the core and periphery.

Somewhere between these two groups is Lisa L. Martin's "International and Domestic Institutions in the EMU Process." This is a useful stand-alone contribution presenting a low-level descriptive narrative of the decision-making process within the European Union (specifically, the *cooperation procedure* under Article 189c of the Maastricht Treaty and the *codecision procedure* under Article 189b). Although well written, it seems out of place next to the complexity (and the assumed prior knowledge) of the other essays.

The book as a whole does contribute to one's understanding of European monetary unification, but it is not for the novice or even for the advanced student in integration studies. A rigorous grounding in econometrics and political economy is assumed (except for the chapters by the editors, Cohen, and Martin); and at $52.50, the book would appear to appeal to specialists and research libraries only.

Cleveland State University LEON HURWITZ

Towards a Theory of United Nations Peacekeeping. By A. B. Fetherston. New York: St. Martin's, 1994. 292p. $59.95 cloth.

There has been a recent dramatic increase of interest in United Nations peacekeeping, a conflict management approach largely moribund and discredited a decade ago. The academic literature has included a large number of new books and articles on the subject, mostly analyses that focus on the new roles assumed by UN peacekeepers in the post-Cold War era or individual case studies of those operations. Fetherston's book is a significant and positive break from this often too descriptive and narrowly focused literature. Her analysis is among the few that seek to understand peacekeeping within the framework of the third-party conflict management, mediation, and negotiation.

The book begins somewhat inauspiciously, and readers may be tempted to quit after the first three chapters; but this would be a serious mistake. Chapter 1 summarizes the history of the United Nations and the development of UN peacekeeping in 19 pages! Chapter 3 is a descriptive treatment of peacekeeping operations in Cyprus, Cambodia, and Bosnia. There is little here that is new or innovative. Chapter 2, however, is useful in pointing out that post-Cold War peacekeeping operations are multidimensional in function, are placed more in protracted, internationalized civil wars, and blur the distinction between peacekeeping and enforcement.

The major and (one might say) pathbreaking contribution of the book is the development in the middle three chapters of the book of a theoretical framework for analyzing and assessing peacekeeping operations. The author advocates the adoption of a contingency model of conflict management drawn from the literature on international mediation. That model is built on the assumption that conflicts pass through several phases and that it is vital to identify intervention strategies that are effective at each stage. To build theory in this fashion is largely an inductive strategy that relies heavily on the accumulation of empirical evidence from studies of conflict management techniques.

There are number of aspects that make this model innovative when applied to peacekeeping and therefore makes it worthy of pursuit by scholars. First, the contingency model allows the analyst to view peacekeeping, peacemaking, and peacebuilding as a single process within a broader conflict framework. This allows both scholars and UN planners alike to see how these elements can be linked or delinked, with accompanying implications for the outcomes of the peacekeeping missions.

Second, the framework permits one to see different levels of the peacekeeping operation (the overall macro level and the operational micro level), seeing how they interact. Too often, there is little integration in the literature between these two levels. The perspective of the soldier or peacekeeper commander does not match well with aggregate assessments of the peacekeeping operation in the broader political context. Accordingly, there appears to be little consensus on what constitutes peacekeeping success and therefore the conditions for that success. This is largely because the literatures at different levels of analysis talk past one another. Fetherston develops a framework to bring these levels together, even if she does not do it fully in this work.

Third, the framework allows the analyst to understand peacekeeping operations' effects on outcomes, together with a variety of contextual factors and processes. Frequently, the current literature ignores these factors, ascribing too much praise or blame to the peacekeeping force for the observed outcomes.

As promising as the contingency model and conflict management framework for analyzing peacekeeping is, there is a notable problem with the classification of a peacekeeping operation as a third-party intervenor. It is quite apparent that peacekeepers fit the role of third parties in traditional peacekeeping roles and several other new roles (e.g., election supervision). Yet this is less clear in other circumstances. Peacekeepers might be better thought of as *primary* parties in those instances (e.g., intervening to support democracy, establishing safe zones, or taking any action involving collective enforcement). Then, the literature on mediation and bargaining suggests strategies, training, and roles significantly different from those in a pure third-party intervention. Because of the wide range of new roles being performed by UN peacekeepers, the framework may not be appropriate to capture all the new mission types.

The last three chapters of the book are a logical extension of their predecessors, but are equally innovative for what they try to do—link training for peacekeepers to the conflict management model. This is another example of how the book develops the connection between the macro and micro levels of peacekeeping. There is a tendency among the many militaries in the world to treat training for peacekeeping personnel as an appendage of traditional military training, the assumption being that basic military skills can be easily translated to peacekeeping duties. A review of existing military training programs by the author confirms that few have specialized peacekeeping training. Yet such training is probably essential, especially if one views peacekeepers as mediators in a conflict process. The author advocates more training in contact skills and cross-cultural sensitivity for peacekeeping personnel at all levels (as military personnel of all ranks perform some peacekeeping, peacemaking, and peacebuilding functions). The argument here that current training programs teach what could be inappropriate attitudes and techniques for peacekeeping is a powerful indictment, and these chapters should be essential reading for military establishments around the globe.

Among current works on UN peacekeeping, there are few that offer a new way of approaching the subject. This study meets that standard; and while the approach is not fully developed and may have some problems in its breadth of applicability, it is a central work of great heuristic value.

University of Illinois,
Urbana-Champaign PAUL F. DIEHL

Allies, Adversaries, and International Trade. By Joanne Gowa. Princeton: Princeton University Press, 1994. 148p. $24.95.

This book begins with an overview of hegemonic stability theory and addresses several criticisms of it. Joanne Gowa illustrates a well-known finding of international trade theory: in highly stylized situations, it will make sense for large economic agents to impose tariffs because the gain in revenues is likely to exceed the costs to consumers. Thus, a country large enough to affect its own terms of trade may impose a tariff increasing its economic welfare beyond what can be obtained through "free trade" while at the same time reducing the economic welfare of the rest of the world. This is known as an *optimal tariff*. Gowa argues that optimal tariffs may be myopic and undercut the hegemonic power from which they derive. In short, along with bribery, taxation, and political concerns, tariffs are one tool that a hegemon may employ. Gowa brushes aside the challenge over whether free trade is a public or private good (based on exclusion) by noting that sanctions are costly—perhaps sufficiently so to overpower the benefits from sanctioning, thus providing an effective barrier to exclusion. Gowa concludes, therefore, that free trade involves public good problems. Further, she argues that the so-called small-group alternative to hegemons, as necessary components of the emergence of cooperation (free trade), may face "insuperable" obstacles to organization and to enforcement. The bulk of these arguments may be found in "Rational Hegemons, Excludable Goods, and Small Groups: An Epitaph for Hegemonic Stability Theory?" (*World Politics*, April 1989). In the conclusion to this chapter, Gowa asserts that the "dearth of free trade among nations over time" is a consequence of the security externalities that serve as barriers to trade. This is a very interesting conclusion but one that does not clearly flow from the tight argumentation in this chapter. I wish Gowa had devoted greater effort to developing and motivating this conclusion, a conclusion that would confront not only standard but also strategic trade theory. It would be an important contribution to develop this argument more completely and directly.

Chapter 3 takes up the theme that strategic interactions among large, rationally allied powers enhances trade among them and reduces trade across alliances, because of the security externalities produced by interstate commerce. More important, Gowa argues that trade among allies produce security externalities (which free up resources), while trade among adversaries involves negative externalities. Thus, in order to enhance national welfare, governments will seek to enhance trade among allies, while not encouraging such activities with adversaries. Gowa's argument is that in situations in which free trade detracts from national welfare, intervention to restrict free trade has the *benefit* of raising the price of imports while at the same time allowing domestic production to expand. Large economies (states) may follow a variant of the optimal tariff policy in order to produce greater national welfare, only if retaliation can be prevented. Gowa argues that the tariff game is a prisoner's dilemma. She illustrates that cooperation (i.e., free trade) can be maintained if the game is thought to be infinite and the discounted sum of future cooperation outweighs the gains from one-shot defection. Gowa suggests that the success of GATT provides an empirical example of this. Between adversaries, Gowa says, a security cost accrues, while among allies, a security benefit emerges. This assumption stands at the core of Gowa's argument. The book would have benefitted from a more direct examination of this particular assertion. If such a negative externality does inure to trade among adversaries, then cooperative free trade requires a higher long-term benefit to be sustained, because the short term benefits of defection are higher. Gowa adds to this the idea that the discount rate is inversely related to the probability of exit and that this is higher in multipolar than in bipolar systems. From this, she deduces that free trade is more likely in bipolar than in multipolar systems, having already suggested that free trade is more likely among allies than among adversaries. This argument stems from Gowa's article, "Bipolarity, Multipolarity, and Free Trade" (*APSR*, June 1983).

Chapter 4 is an empirical examination of annual trade volume among a small number of large states (the United States, Great Britain, France, Italy, Germany, the Soviet Union, and Japan) as a function of per capita gross national product, distances, alliances, and war for a series of cross sections from 1905 to 1988. So far as I can determine, these results are identical to those in her article with Edward D. Mansfield, "Power Politics and International Trade (*APSR*, June 1993). I am puzzled why Gowa reported these earlier results, rather than take the opportunity to replicate them in some different context, that is, with more recent and/or more exhaustive data. Why not also disaggregate the trade model into specific sectors in which the effects might be more evident? The findings themselves suggest considerable variability in the magnitude, sign, and significance of the alliance variables. There is greater statistical impact of alliances on trade in more recent periods (which happens also to be the bipolarity periods) and the greater impact of bipolar, as compared to multipolar, alliance pacts. The analysis does not clearly flow from the earlier chapters. In particular, there is not attention to the question why trade volume should be related to free trade, except in a very general way. Why would a research design focused on a gravity-based trade model tell us much about the incentives and disincentives for free trade? This seems quite orthogonal to the underlying argument. Why not use tariff revenue data more directly to test the argument? In empirical studies, tariff barriers do not always turn out to be significant barriers to sectoral trade. Gravity models themselves do not derive from standard macroeconomic assumptions (e.g., equilibrium) but are more empirically determined. Note that both bilateral trade (the dependent variable) and the gross national products (among the independent variables) are actually determined simultaneously in equilibrium, which creates conceptual, as well as statistical, problems (endogeneity). One solution might be to instrument the national product with factor endowments. However, there is an additional problem with using gross national product instead of gross domestic product on the right-hand side, because it includes the trade that is on the left-hand side. Moreover, there are some problems with simply adding variables to a gravity model without taking into account the terms of trade implied by the addition of those variables. The question "Why does trade volume tell us anything about the tariff game?" also needs to be addressed. My greatest concern, however, is that by focusing on total *trade*, we ignore how international commerce really works: firms buy and sell products and services. Total trade is simply an aggregation of this stream of events. This returns us to the optimal tariff problem. The optimal tariff only makes much sense in a world in which there are a small number of goods and players (typically, two each) and in which the market size of one player is substantially larger than the other. In a multisectored world of many large players each competing in the same sectors and each commanding a sizable domestic, as well as international, market in the same goods, it is not clear that optimal tariffs are very plausible.

Chapter 5 is a case study of the Anglo-French Entente, an *entente* that existed in the pre–World War I era, during which Gowa concluded that alliances did not exert an important effect on bilateral trade. In this chapter, Gowa unpacks British decision making with an eye to whether it conforms to the notion of security externalities developed earlier. It seems clear that British policymakers acted in ways significantly different from those postulated under Gowa's model. In particular, they perceived the gains from free trade to be rather minor, insufficient to offset the transaction costs. The combination of systemic, coherent argument with a quantitative, as well as qualitative, probing of some of the consequences of that argument is compelling and important. The results of each investigation are in some ways at loggerheads with each other, and do suggest future research. On this basis, Gowa concludes that both politics and economics are important and intertwined in international relations.

Despite my reservations, this book is very tightly put together and addresses an important topic, one at the core of international political economy. Readers will come away from reading the book knowing more and having been stimulated to think carefully about the link between trade and international politics. Gowa not only is conversant with the literature from political science but also fruitfully brings to focus macro- and microeconomic tools and perspectives. I think that there is a great deal of work yet to be done in this area, and Gowa deserves our thanks for reenergizing the investigation of trade and conflict and cooperation. She has presented a tight, deductive argument that is annealed by exposure to both quantitative and qualitative analysis.

University of Colorado, Boulder MICHAEL D. WARD

Improving Human Rights. By Michael Haas. Westport, CT: Praeger Publishers, 1994. 247p. $24.95.

Improving Human Rights aims to understand "why human rights are protected in some countries rather than others" (p. 15) through an exploratory cross-national statistical analysis. Unfortunately, that mission fails because a massive data base is paired with a rather primitive research design that lacks the theoretical grounding required to focus the analysis on key questions. Nonetheless, because of the other contributions this book makes, it should be consulted by quantitative researchers investigating human rights practices.

The volume opens promisingly with a brief history of human rights concerns and an exhaustive survey of efforts to conceptualize human rights and to gather data concerning the human rights practices of nations. Thereafter, the early portion of the book is devoted to creating multivariable dimensions upon which predictor variables can then be regressed.

His analysis contains 50 indicators of civil and political rights, 91 indicators of economic and social rights, and 57 "predictor" variables, all gathered for a cross section of 88 countries in 1982–83 and 1985–86. Civil and political rights are culled principally from a reading of the International Covenant on Civil and Political Rights, the text of which is included as appendix A. The range of rights considered is very broad, including, for example, the right to free legal aid and freedom from compulsory military service as well as from capital punishment. He does not define coding procedures, many of which appear to require considerable subjectivity. Economic and social rights are gleaned largely from the International Covenant on Economic, Social, and Cultural Rights, which forms appendix B. The resulting variables include such aggregate economic indicators as unemployment rate, improvement in terms of trade, and consumer spending per capita, as well as measures that lie closer to standard interpretations of economic rights (e.g., the freedom to join independent trade unions). Haas also includes variables that "are hypothesized to predict why some countries observe human rights better than others do" (p. 46), including measures of economic development and equality, political system attributes, government spending, and domestic conflict.

Factor analysis and a clustering technique are used "to determine whether human rights ratings cluster together on just one empirical dimension or comprise several empirically distinct clusters of rights" (p. 51). The results, which are presented in nearly 40 pages of figures and tables, indicate that while nearly all rights were positively correlated with all others, distinctive patterns of association suggest a clustering of rights. He correctly concludes that "efforts to find a single number in order to rank countries on the extent of human rights observance are clearly misguided" and that "many *a priori* categorizations of human rights are inconsistent with the data" (p. 63). Future researchers would do well to embrace this well-documented finding.

Beyond this basic data reduction, Haas ventures occasionally into interpretations that do not seem wholly justified by the analyses themselves. For example, he concludes that "judicial rights and privacy rights develop only after countries observe prisoner's rights" (pp. 56–57), but it is not clear how he reaches this conclusion. While few would question his contention that "middle class heterosexual males of the dominant ethnic group . . . gain human rights before . . . females, ethnic minorities, workers, and sexual minorities" (p. 63), no explicit test of the sequencing of rights is conducted. Indeed, he does not formulate particular hypotheses or design concrete tests of the claims that have been made elsewhere in this literature. This is both surprising and disappointing, because earlier, he had provided very brief references to a dozen or so theories, with the central perspective of each described in a few sentences seemingly fashioned to highlight the possibility of reducing each to a falsifiable hypothesis.

Instead, he simply reports a series of regression analyses utilizing the dimensions extracted from the factor analyses of rights and predictor variables. He makes no reference to standard regression diagnostics, nor does he display any sensitivity to the difficulties of inferring casual relationships from correlational analysis. Without explicit guidance from theoretical claims, Haas allows the data to speak for themselves. Frequently, the message they convey is interpretable as supportive of various theories of human rights determination; but too often, the results are less coherent. Amid the dozens of relationships he reports are the following: "Lower heart disease deathrates in 1982 best predicted decreased restrictions on foreign travel"; "11 percent of the variance in 'disappearances' for 1982 was explained by the number of deaths due to domestic violence"; and "Reduced military spending from 1982 to 1986 explained 6 to 7 percent of advances in rights for gays and Lesbians" (pp. 77–81). (As elsewhere in this volume, the style suggests a research report, rather than a discursive probe of alternative theoretical ideas. The book devotes less space to text and footnotes than to figures and tables.)

Given that the title of the book is *Improving Human Rights*, the absence of any effort to model processes is a surprising weakness. While the results reported here are not irrelevant to this goal, the research design does not focus explicitly on how this can best be accomplished.

Lehigh University BRUCE E. MOON

Governing Capitalist Economies: Performance and Control of Economic Sectors. Edited by J. Rogers Hollingsworth, Philippe C. Schmitter, and Wolfgang Streeck. New York: Oxford University Press, 1994. 328p. $45.00.

The rise of new industrial competitors in the last two decades has prompted a significant revival of Andrew Shonfield's interest in the comparative forms of capitalism. *Governing Capitalist Economies* provides an important contribution by bringing together a first-rate set of chapter studies with a range of theoretical considerations by the editors.

The editors use the term *governance* less in a generic sense than in the technical sense of coordinating economic transactions. They extend the Williamsonian dichotomy of market versus hierarchy to include other governance structures (networks, associations, and communities) that they have done much of their other writings to delineate. Not only do the organizational forms of economic governance vary across countries, but the indices of industrial performance may also differ from country to country. Based on such considerations, the editors define a governance system as "the totality of institutional arrangements—including rules and rule-making agents—that regulate transactions inside and across the boundaries of an economic system" (p. 5). Using this broad approach, the editors address several major issues in the nature of contemporary capitalism: the salience of countries versus sectors, the effects of inherited social institutions on subsequent economic performance, and the forces militating toward, as well as against, convergence of domestic institutional arrangements in the advanced countries.

Given such a broad framework, it is especially appropriate that the empirical chapters are themselves explicitly comparative. Each author examines the goals and mechanisms for coordinating transactions within a single sector in at least two countries. This inductive plan includes consistently high quality chapters on electronics, steel, shipbuilding, machine tools, chemicals, autos, dairy, and the securities industries. The chapters by Mari Sako on printed circuit-boards and Gary Merrigel on machine tools are particularly interesting because they push the editors' theoretical concepts while also providing a great deal of empirical detail on interfirm relations in their respective sectors.

The main contribution of the concluding chapter, by Hollingsworth and Streeck, concerns possible scenarios of convergence or continued diversity in the institutional structures of different countries. These two editors argue that "differences in governance within sectors are often recognizable as national differences in that they follow a similar logic across sectors" (p. 272). In short, sectoral effects are subsumed within country effects. From this conclusion, the authors enumerate a number of reasons why diversity in national patterns of economic governance may well persist in the face of pressures toward transnational harmonization. Not only are current governance institutions slow to change, but national industries may find distinctive market niches or functionally equivalent strategies for competing in international markets while retaining particular domestic arrangements.

One issue that the conclusion treats less persuasively concerns the subtitle's reference to the performance issues raised by advanced capitalist competition. By allowing that different performance criteria may prevail in different countries, the editors tolerate a good deal of analytic freedom. There is also a revealing analytic tension here. On the one hand, the volume's economic approaches (both transaction-cost and industrial organization perspectives) rely too heavily on efficiency-driven analysis. On the other hand, the volume makes little effort to deal with the issues treated by management scholars under the rubric of firm-level heterogeneity. Comparative political economists typically argue that the relative capabilities of countries are best grasped by comparing sectors, while management scholars usually argue (from single-country data) that firm-level variations and managerial choices outweigh any patterns evident at the level of industrywide aggregates.

The editors of this book should hardly be expected to resolve the entire debate. The point is rather that more attention to the existence of intercompany variation might have forced the contributors to compare more precisely the mechanisms by which governance institutions lead to performance outcomes and to ask whether they do so through firm-level effects or industry-level effects.

A second issue—raised but not resolved—concerns the possible erosion of national institutional differences by multinational firms. The conclusion cites Michael Porter and other management scholars in suggesting that multinational firms retain strong ties to their headquarter's country. Yet other management experts, particularly Christopher Bartlett and Sumantra Ghoshal, have emphasized the ability of multinationals to build network organizations without any single country base. Here, again, the "heterogeneity" of firms is important because these organizational forms themselves become intangible "resources" at management's disposal. Interactions among such managerial choices, sectoral institutions, and government policies therefore become one of the pivotal areas of desirable interchange between political economists and management specialists.

In this sense, *Governing Capitalist Economies* does precisely what all provocative edited volumes should do. It synthesizes several strands of recent literature, frames a group of first-rate empirical studies, and raises questions of central importance for future research.

Massachusetts Institute of Technology J. NICHOLAS ZIEGLER

Peripheral Visions: Deterrence Theory and American Foreign Policy in the Third World, 1965–1990. By Ted Hopf. Ann Arbor: University of Michigan Press, 1994. 306p. $49.50.

This is an example of what can be very intellectually valuable and stimulating about critiques of deterrence theory but also of what can be wrong with such critiques. A reviewer's concern arises in comparing this book's dust jacket, which tells us that "Ted Hopf repudiates the core assumptions of deterrence theory", with the book's page 233, which reads "The basic core of the theory remains sound". We have too many instances, in general, of authors "disproving" the theory of deterrence but only by attacking something somewhat different. What Hopf refers to throughout this book as "deterrence theory" is mostly "domino theory"—the assumption that a defeat on a minor front will lead adversaries to question one's resolve and then to attack on many other fronts. In a close analysis of Soviet public statements interpreting a series of American foreign policy moves—from the end of the Vietnam War to the Reagan administration's intervention in Grenada—Hopf makes a case that American retreats were not typically seized upon as a sign of weakness and thus that the U.S. military effort was not as necessary as "deterrence theorists" thought.

Hopf admits that there may be problems with attaching great significance to the Soviet ideological pronouncements here—Leninist pronouncements by which the United States is always dangerous and aggressive and by which the flow of world events is historically determined, rather than a simple contest of wills. A skeptic about the importance of ideology might have seen this as mainly window-dressing for the power struggle with the West (a contest that the Soviets lost, in what struck many observers in 1989 as a dramatic illustration of "domino theory" in reverse).

In thus attacking "deterrence theory," Hopf then introduces another issue (which is, however, somewhat orthogonal to the falling-domino metaphor), namely, whether economic power is not more useful and relevant than military power—one of the generalizations most often presented now at the close of the Cold War.

The author is surely correct that economic power may have rescued the West from military weakness in face of the supposed power of the Warsaw Pact in the 1970s and 1980s and in face of the U.S. failure to win the military contest of resolve in Vietnam. Yet as he notes in the more carefully phrased concluding portions of the book, this is hardly to question the core of the logic of deterrence, that is, "to increase the perceived costs of adventurism to a potential challenger" (p. 236).

Contrary to the general thrust of Hopf's analysis, no deterrence theorist would have denied that economic retaliation and penalties can be part of the appropriate deterrent array here; after all, West Berlin and Hong Kong were for decades shielded by economic deterrence.

But economic pressures and sanctions can become enmeshed in contests and failures of resolve just as easily as military confrontations—the problems of the Clinton administration in facing up to Serbs and North Koreans being all-too-apt illustrations.

If one gives up in an economic contest or a political contest, one will thus hear all the same arguments as were heard on the military contest in Vietnam—that the adversary will then "nickel-and-dime" us further, that other adversaries will be emboldened, and so on. Hopf's evidence certainly shows how such a "slippery-slope" pattern can sometimes be turned around, but he is less convincing in his assertions that the exaggerated "domino" fears are to be found exclusively or predominantly where military tools are involved.

Much of deterrence theory is indeed too often left at the level of intuition, and Hopf's cross-examination of the logic here performs a valuable service in nailing down strategic propositions that pass too easily for common sense.

But, as noted, a questioning of any particular aspect of deterrence is too often greeted as the repudiation of all of deterrence theory. The total of what is "repudiated" here, after all this careful analysis, may be not be so much.

University of Maryland GEORGE H. QUESTER

Strategic Studies and World Order. By Bradley S. Klein. Cambridge: Cambridge University Press, 1994. 196p. $49.95 cloth, $16.95 paper.

One of the primary aims of a positivist social science is to explain and predict human behavior. Positivists generally believe that explanation and prediction are possible only where the scientist "objectifies" the social conduct in question, treating it in terms that presume its independence from the scientist. As a result, in positivism, theory is seen as external to social practice, as an object separable from the domain it explains.

Many antipositivists are concerned less to deny the feasibility of such an externalizing or objectifying strategy than to argue that the unconscious pursuit of this strategy can have pernicious effects. For such critics of positivism, social theory is not just *about* reality, it is *of* it; and to try to externalize theory from practice is to mystify its constitutive social role. Theory cannot be separated from the domain it explains. In this view, positivism's objectifying strategy of causal inquiry inevitably masks or distorts the true nature and impact of social science.

Bradley Klein's book falls squarely in the antipositivist camp. Klein considers a range of concepts in strategic studies (e.g., deterrence, balance of power, alliances, and the states system) not as the basis for knowledge about the world but as "cultural constructs" that need to be historicized and relativized through genealogical analysis. (Foucault is an important influence here.) "The issue, in short," the authors writes, "is not whether [these constructs] are true or false but how they have acquired their meaning, and how that meaning has changed to sustain the shape of contemporary political life" (p. 10).

The book traces the historical development of a number of central themes, terms, and problems in deterrence theory and strategic studies. The first two chapters offer theoretical preliminaries. Chapter 3 develops the classical principles of warfare in a critical (and highly perceptive) discussion of Clausewitz. Two chapters are then devoted to a number of familiar topics, many connected to the development of nuclear weapons, including the debate between "minimalists" and "maximalists" in nuclear strategy during the Cold War, the dynamics of American power projection in the twentieth century, and the politics and problems of the NATO alliance. A concluding chapter considers the winding down and ending of the Cold War. One of Klein's main concluding points is that the end of the Cold War has made it more important, rather than less, to understand the scope and influence of the Western strategic project. Throughout, Klein's treatment of the positivist literature making up strategic studies is concise, informed, and fair.

Unlike a positivist, however, Klein emphasizes the constitutive role of strategic studies in world politics. Seeing past theory as embedded in present institutional arrangements and forms of legitimacy, Klein asserts that "long-standing practices of militarism have . . . become insinuated into the quotidian fabric of life" (p. 42). During the Cold War, Klein believes, "nuclear strategy was . . . part of an ongoing social process of Alliance construction, definition, articulation and delimitation. It was part of a broad postwar strategy of securing the spheres of social reproduction required for maintaining the American—and Western—way of life" (p. 112). Through these and similar judgments, Klein's critique of positivist (objectifying) strategic theorizing takes shape.

The main problem with Klein's work is that this constitutive account is inadequately spelled out. Very little attention is given to the processes through which strategic studies have helped shape the world. To make this criticism is not to suggest that Klein should have offered a positivist theory within the field of strategic studies (a theory of deterrence, the balance of power, and the like). Rather, it is to say that Klein's antipositivist assessment of strategic studies itself invites a causal judgment that Klein fails to vindicate. Klein's aim, after all, is to show how existing theories and research programs in strategic studies have shaped the world. To argue that a positivist theory has social effects is to make a fairly straightforward causal claim. The analyst suspends the positivism (the external standing) of the first-order theory, so to speak (e.g., balance-of-power theory), in order to make a second-order causal argument (e.g., that balance-of-power theory has helped produce the states system). It is unclear why such second-order claims should remain immune to empirical test along roughly positivist lines. For example, Klein asserts (in an echo of the book's major thesis) that "the once-dominant International Relations discourse of realism . . . helped contribute to the legitimacy and persistence of a world dominated by major powers" (p. 35). This sounds very much like a causal claim that a positivist might be able to help assess.

The import of this criticism can best be seen in considering Klein's closing argument. For Klein, the end of the Cold War properly entails not just a rethinking of strategy and deterrence but a confrontation with "the broader representational repertoire which make[s] strategy and deterrence possible in the first place" (p. 140). Yet without some account of how representations or categories of thinking yield or constrain strategies of action—how the theories that social scientists invent help to constitute and transform the world—the implications and even the meaning of this fundamental shift in strategic concepts remain unclear. While it may be true that positivist theorizing offers little guidance in times of conceptual dislocation, the reader is left with little reason to think that a one-sided antipositivist approach will fare much better.

College of William & Mary David Dessler

In from the Cold: National Security and Parliamentary Democracy. By Laurence Lustgarten and Ian Leigh. Oxford: Oxford University Press, 1994. 554p. $49.95.

In this large and dense but well-written volume, Lustgarten and Leigh (both members of law departments in British universities) address the coexistence of national security and parliamentary democracy, specifically with respect to values such as "privacy, freedom of expression and freedom of movement" (p. viii). The book, reflecting its authors' experience, focuses upon the United Kingdom, although discussions of Canada and Australia are also included. As the title suggests, they discern a new openness within the United Kingdom with respect to national security issues, and the book contains information obtained for the first time from security service personnel.

The book consists of 17 chapters and comprehensive lists of cases and treaties. The chapters are grouped into five parts: "Conceptual Foundations," "National Security and Human Rights," "Criticism, Dissent, and National Security," "National Security and the Legal Process," and "Controlling National Security Institutions."

The authors take as their starting point Chuck Tilly's insights concerning the relations between war making and state making. From this, they rightly deduce that representatives of the state apparatus will typically place issues of national security above issues of individual and group freedom. Moreover, the authors note at one juncture, "those who are responsible for the national security must be the sole judge of what national security requires. . . . It would obviously be undesirable that such matters be made the subject of evidence in a court of law or otherwise discussed in public" (p. 326).

The ramifications of this are many. On a practical level, it means that state organizations can operate in a tautological vacuum, with the national security being defined in any way that the state chooses. For instance, as Lustgarten and Leigh observe in chapter 14, the definition of terrorism rests with the security forces: once they have determined that an individual or collective is beyond the efforts of the police to manage, then more elaborate steps can be taken to control them. However, as the police in the United Kingdom have very limited abilities to screen or bug premises, hard information on those suspected is often not collected *until* their stature as terrorists has been asserted. The authors point out the absurdity of this reasoning and underline the loss of legal recourse for those who fall into the web of the security forces; the very existence of some is denied by successive British governments (p. 374).

Lustgarten and Leigh clearly believe that it is possible to create some situation in which these extremes are brought back under parliamentary control. However, I remain skeptical on two counts. The first can be derived from their own analysis, which links the emergence of the state back to basic questions of war making. It should not be forgotten that the United Kingdom has been involved in two major struggles in recent years, one in the South Atlantic, the other in Northern Ireland. The second alone has been serious enough to allow the state's representatives to pursue extreme actions in support of the national interest. The second issue is the manner in which the national interest itself is a shifting target. At different moments, different threats will cause national security to be defined differently. For example, in their discussion of the British Broadcasting Corporation (BBC), the authors contrast the unswerving loyalty displayed between 1939–45 with the stance of the BBC during the Falklands/Malvinas crisis. They argue that because the conflict did not threaten the stability of the country, the staff were able to be critical of the persecution of the war. This is perhaps true of domestic television; but as one who listened to the World Service radio reports during 1982, I remember that there was no question that the BBC promoted an intensely anti-Argentine line for international consumption. In the final analysis, the state's broadcasting agency will always support it. Indeed, it is ironic that this volume's dustjacket displays a handsome photograph of Orson Welles as Harry Lime in *The Third Man*. In the movie, Lime was a black marketeer and subversive sought by the security services in Vienna; but when the BBC remade it into a television series (at a time when British forces were still in Europe), he became, of necessity, something of a hero in the Robin Hood tradition.

In short, the authors argue that there is the possibility of creating a legal control of the security system. In my view, this cannot succeed: there will always be spillover effects from nation to nation, with espionage (now often of the industrial variety) and other tensions generating security crises. The only context in which the state apparatus will ever relinquish its control is with the recognition that the issues at stake are no longer of importance. But that would be an admission that the nation-state in question has no stature, and (as we see frequently) small countries are often keener to assert their importance than their larger neighbors. This aside, *In from the Cold* is a valuable and provocative book. It is scholarly and well written and presents new benchmarks against which the topic will have to be evaluated in the future.

Arizona State University West ANDREW KIRBY

International Event Data Developments: DDIR Phase II. Edited by Richard L. Merritt, Robert G. Muncaster, and Dina A. Zinnes. Ann Arbor: University of Michigan Press, 1993. 248p. $39.50.

This volume represents an attempt to emerge from what the editors acknowledge to be the "hiatus in event data research during the 1980s" to report on the enhancement of existing data sets and the development of related computer software. One contribution of the volume is a useful history of event data, including the coordination of many present efforts through the Global Event Data System project at the University of Maryland.

Six major active event data research projects are described. These are Davies, Gurr, and McDaniel's COPDAB; Howell's related SEAS; Sherman's SHERFACS; Leng's BCOW; Alker's Interpretive Analysis; and Schrodt, Francisco, and Gerner's KEDS. One asset of the volume is the presentation of the evolution of what might be termed a research program. The chronicling of the various stages of the event data scholarship can give us some insight into a group that is overtly trying to "progress."

In their overview chapter on GEDS, Davies and McDaniel point out many of the innovations of GEDS that respond to early criticisms of event data. These include the coding of major nonstate entities, and source referencing that allows a potential user to decide from what perspective an event has been coded. They also report on the technological advances in event data research that have allowed substantial software development and an increase in automation.

Source coverage of events, especially as it deals with nation-states of the South, has long been a problem for event data. The issue is addressed directly by Howell and Barnes. In their short piece, they provide evidence for the more even-handed coverage of Reuters over other event data sources.

Tomlinson examines WEIS data, and concludes that "conventional conflict-cooperation continua are simplistic and suggests an alternative constructive-deconstructive conflict-terminology to convey the notion of complex events" (p. 55). Through vector algebraic representations reminiscent of Quincy Wright's work, Tomlinson argues for a conception of reciprocity and dispute resolution similar to those of Patchen, Goldstein, and Freeman.

In presenting their work with the SHERFACS data set, Sherman and Neack assign to themselves the difficult task of trying to "bridge the gap between structural attributes and behavioral explanations of conflict escalation and settlement" (p. 87). In trying to isolate structural factors that differentiate between nonhostile and deadly international conflict since World War II, they focus on a variety of independent variables. This quite promising chapter becomes confusing. For example, one conclusion is that "substantive issues" are one of the "best indicators of phase trajectory groups" (p. 87). Since "phase trajectory groups" are the operationalization of the dependent variable of the conflict stage, I hoped for a careful discussion of which categories of substantive issues characterizing conflicts were most likely to be typical of escalating conflicts and under what circumstances. Despite a table of results and a brief summary interpretation, no such discussion ensued. Sherman and Neack's attempt to follow a counterfactual path to examine "new world orders" is again promising but truncated.

Leng and Schrodt present consecutive chapters reporting their separate projects designed to automate the coding of event data. Leng's chapter on BCOW describes his approach and then optimistically concludes, "Over the long run, it may well be that the data generation process can be automated fully with no loss in validity" (p. 122). In describing the WINR and KEDS systems in some detail, Schrodt reaches what seems to me to be the more realistic conclusion that "the machine coding of machine-readable news sources provides a practical and low-cost alternative for generating event data" while admitting that "such data will generally be of lower quality than human-coded events" (p. 126). Schrodt carefully illustrates the specific trade-offs between quantity and quality and leaves the potential researcher to make an informed choice.

Alker's chapter "explores several frame-based ways of automating preliminary interpretive codings of conflict management relevant information generated by the GEDS system" (p. 149). In taking this approach, Alker recognizes the crucial feature that early event data analyses ignored: codings of event data are interpretive, not objective. After critiquing some features of what he terms "conventional event data," Alker sets out to improve upon it. His two major reported undertakings in this direction are "improving on DDIR/GEDS events coding using a mature Laswellian action/interaction coding framework" and "redefining the Schank-Abelson-Reisbeck repertoire of interpretive routines in a Laswellian events framework" (pp. 154, 156). One would expect no less from Alker. The material in this chapter is dense and quite suggestive of the very innovative directions in which the author is heading. One is left with curiosity piqued for further elaborations of this work.

Seitz concludes the volume with a lengthy piece titled, "Through Rose-colored Glasses: Computational Interpretations of Events." In it, he proposes "a strategy for the systematic analysis of textual material that differs from semantic parsing and hermeneutic contextualism" (p. 169). His proposal is an incredibly ambitious one; and its most crucial element is the establishment of "libraries of interpretive frameworks," that is, metaphors and parables from which one can work backward to the structuring of text streams. As I see it, the most difficult challenge of such an approach is having a voluminous enough library to help with cross-national interpretive research. This may or may not be possible. I am certainly not yet willing to agree with Seitz's statement that "a relatively small number of interpretive frameworks can cover much of the textual material of general interest to political scientists" (p. 219). For a particular U.S. administration dealing with foreign policy, one might argue that the set of potentially applicable metaphors or parables would be specifiable. This becomes more problematic for other international relations research tasks that have a broader scope. Like Alker, however, Seitz has recognized the crucial interpretive hurdle for event data and is grappling with it creatively.

In summary, this is a helpful volume for those who seek to understand some of the empirical options available in applying event data to international politics research puzzles. It provides data and analyses more comprehensively than in most comparable volumes. It also gives the reader a flavor of the new and often creative directions of the once almost-dormant use of event data.

Ohio State University DONALD A. SYLVAN

Disconcerted Europe: The Search for a New Security Architecture. Edited by Alexander Moens and Christopher Anstis. Boulder: Westview, 1994. 271p. $56.95.

It is very difficult to write a book about institutions when they and the circumstances that are supposed to give them their purpose are both in flux. This, however, is the task that Moens and Anstis, two Canadian academics with external affairs backgrounds, set themselves and their mostly non-American contributors in this study of the struggle to develop a "new security architecture" for post–Cold War Europe. In doing so, they provide a valuable guide to the plethora of agencies, conferences, and initiatives that have emerged since 1989. There is, for example, a glossary listing nearly 60 acronyms (still not comprehensive); and several of the contributors provide narratives that help recall the sequence in which many these developments emerged. The problem is, of course, that with events as recent as these and issues that remain as yet unresolved, we have no way of telling how much of the detail is going to be worth recalling in the future.

The book is divided into two sections. The first identifies the problem—that after a sustained diplomatic effort, there remains no agreed institutional framework for dealing with very real threats to European security—and charts the institutional efforts made so far. Charles Krupnick looks at the Cold War background, while three essays by Moens, Anthony Forster, and Anstis deal with the new NATO, the European Community (now Union) and Western European Union (WEU), and the Conference (now Organization) on Security and Cooperation in Europe, respectively. The second section examines the policies of the great powers: Krupnick on the United States, Forster on Great Britain, Roy Rempel on Germany, and Anand Menon on France. Italians, I am afraid, will be offended. The section concludes with an essay by the editors, in which they present a very tentative proposal (just over four pages) regarding what should be done. A more pragmatic approach should be taken, they argue, in which primarily the Western Europeans and the North Americans work out the procedures for, and commit the resources to, securing the borders of those eastern and southern European states which commit themselves to respecting the rights of their national and ethnic minorities.

The reasons for the present mess are clear for anyone to see. The collapse of the Soviet threat in any plausible form destroyed the will and resolve of what the editors still call "the Western Allies" to act in concert when the costs or risks of so doing are anything but minimal. Of course, it is more complicated than that. As several of the contributors point out, the interlocking character ascribed to the multiple institutions of European security by member governments has been rarely seen to work in practice. Instead of a division of labor between, for example NATO and the EU/WEU in the Balkans, we have witnessed competing claims for institutional authority in particular situations followed by buck-passing when the political will to commit resources has been found to be lacking. This competition has also been fueled by the longstanding ambitions and fears of particular states. The Americans want to do less without a corresponding loss of influence over what gets done; the French want a European dimension in which their voice is more effectively heard, and so the British do not; and the Germans wonder how they can at once be more influential without offending anybody—not least, a considerable portion of their own public.

One might suppose that all this constituted a sufficient prescription for paralysis. The unifying theme of the book, however, is that the failure to agree to a common institutional framework within which to adopt coherent security strategies is itself an important source of problems. Institutions matter; get the institutions right, therefore, and other things will start to work. This may be so, but it begs the question of how the consensus about institutions is to be achieved. Given the catalogue of disappointments through which we have just been guided in great detail, why should we accept the suggestion of the concluding essay that conferences designed to link certain more modest political objectives with the combined joint task force initiative "could well create a common political game plan into which a concrete military framework can be fitted" (p. 242)? In the absence of a clear and present common danger, carrots and sticks remain the basis of effective collective action; and clearly, none of the great powers has enough of either to secure the cooperation of its fellows.

The brevity and cautious character of the editors' proposal suggests that this book was written to be taken seriously by the professionals. Neither sweeping five-point plans for European (hence, world) security in the twenty-first century nor sparse testable hypotheses are to be found here, only a detailed and at times dense analytical narrative based mainly on a combination of press reports, documents on the public record, and interviews. The approach does have its drawbacks. On occasions, it results in the contributors' becoming collaborators in sustaining the opacities that are the stock-in-trade of the people they are studying. At one point, for example, we are told that Yugoslavia shows the need for "a CSCE norm-setting institution" and that the CSCE "has failed to galvanize the political will need to shake governments out of their traditional ways of thinking" (p. 108). At another, official platitudes about how Germany's voice needs to reflect Germany's new weight while Germany's new weight needs to reflect the historical sensibilities of its neighbors are let past with little comment.

A more serious shortcoming with the approach is revealed in the second section of the book dealing with the policies of the great powers. Each country is presented as struggling with its own uncertainties and lack of means in a universe where everyone else's purposes seem far more clearly defined. Britain, according to Forster, is trying to do everything with less and struggling to keep up, while Menon's France is reluctant to renounce its Gaullist past and thus risks becoming isolated. Even Germany, according to Rempel, is trying to maintain its combination of Atlanticism and European federalism but faces great challenges. But by what or whom is Germany to be challenged, France to be isolated, and Britain left behind? Certainly not by the effective collaboration of their fellows if these conclusions and the general argument of the book are correct. This puzzle results, in part, from the edited character of the book, but it is also a consequence of continuing to regard great power collaboration and the institutions by which it is achieved as the principal point of focus of both scholarly and professional concern. The failure of the great powers to collaborate effectively is frequently ascribed to the failure to adjust their thinking to the new circumstances of post–Cold War international politics; but perhaps the preoccupation with collaboration and the associated expectation that great powers ought to collaborate is the real Cold War anachronism.

University of Minnesota, Duluth PAUL SHARP

International Organization and Industrial Change: Global Governance since 1850. By Craig N. Murphy. New York: Oxford University Press, 1994. 357p. $39.95 cloth, $17.95 paper.

International organization in the modern world (from 1850 to the present) can be explained as the institutional means through which privileged nations have confronted the contradictions inherent in the evolution of the industrial-capitalist world order. This, in a nutshell, is the central thesis of this well-written and extensively researched treatise on the history of international organization over the past two centuries. The analytical focus puts the work squarely in the leftist (i.e., Marxist-Gramscian) intellectual tradition explaining institutions. At the most general level in the theory of politics, the work bears on the origin and nature of the state. At the level of international relations, the work bears on the question of whether supranational organizations are in fact the institutionalization of hegemony (i.e., a legitimating device for the predominant mode of social relations embodied in the configuration of power in the international political economy). The author scans five periods in exploring the relevance of what essentially is a functionalist (not to be confused with Mitrany's functionalism) vision of international organization. International organizations are born out of the imperatives of the capitalist world order and serve very specific functions with respect to preserving this world order.

The author begins with the rise of supranational institutions during the industrial revolution (i.e., what he calls the interimperial world order), 1850–1914. The industrial revolution created pressures to provide ever-expanding markets for capitalism's capacity for mass production. International unions were principal mechanisms through which industrializing nations joined to produce a growing international market for these goods. They essentially reduced the transaction costs of integrating markets by providing common standards by which to travel, measure, and communicate. As good as the expanding markets (and the new products that these markets made possible) were for perpetuating the industrial revolution, they also carried destabilizing elements for the capitalist world order by encouraging economic competition for markets and resources (i.e., lateral pressure) and encouraging uneven development among the leading economies. These contradictions culminated in World War I.

The interwar period saw little success in building supranational institutions that would break down the walls of economic and political nationalism forged by the war and reinforced by an ideological shift toward a welfare state mentality. The League of Nations failed to reintroduce the stability of the interimperial period because its institutional structure deviated from the configuration of power in the international system (i.e., the rules of collective security could not deter expansionist nations). With the conclusion of World War II came the high point of the period of the free world order (1945–70). The great capitalist compromise of this period, the leadership of the United States, and the impact of international organizations brought back both the prosperity and stability of the interimperial order. The scope of international organization in this period increased beyond its traditional role of facilitating exchange between capitalist economies and addressed problems of development, peacekeeping, human rights, and public finance. The crises of the 1970s and 1980s undermined this great compromise with new challenges: the stagflation of the 1970s, the slow growth of the 1980s, declining U.S. hegemony, the counterideology of the new international economic order, the debt crisis, and antilabor policies in Organization for Economic Cooperation and Development (i.e., the privileged) nations. This renders prospects for the final period (the future) problematic for the author. The book ends with a discussion of competing visions for managing the future world order (a liberal versus a Keynesian-Marxist style) in the context of what the author considers the crucial issues in the years to come—especially the environment, security, and development.

The sweeping historical coverage, analytical focus, and the boldness of the assertions are commendable. In a field in which scholars tend more and more to scrutinize only the individual trees, the author of this book forces us to look at the forest. The work is also very clearly written. Considering that this scholarship comes out of an intellectual tradition that has raised the use of bombastic prose to an art form, this is more than just trivial praise. As with most ventures that take such a broad aerial view with a singular explanatory logic, however, this one, too, has difficulty accounting for the diversity of outcomes in the international political economy over the past two centuries. In explaining the devolution of the capitalist world order during the interwar period, for example, the author betrays his Gramscian roots when he relies on more conventional arguments gravitating around geostrategic factors (i.e., ongoing competition between Germany and the victors of the war) and domestic political factors (the new economic nationalism). Moreover, much of the logic accounting for developments in the post–World War I free world order smacks of hegemonic stability. The interwar years owed much to the lack of a champion willing to provided the public goods necessary for the smooth functioning of a capitalist world economy. This changed with the great compromise after World War II, in which the United States voluntarily took on this role. The subsequent crises of the 1970s and 1980s occurred in a period of declining U.S. leadership. Interestingly, international unions, according to the author, were quite successful in keeping the capitalist world order on a steady course during the interimperial period without much of a commitment to lead from Great Britain. But here, differentiation between institutional regimes that succeeded and those which failed suggests another explanatory factor—the structure of incentives underlying relations across various issue areas. The institutions that were most successful were essentially solving coordination games (i.e., standardizing communication, transportation, and measurement). These games require very little institutional support, because norms are self-enforcing. However, in more redistributional games, institutional regimes either did not appear or experienced limited success. Trade was not regulated by a multilateral regime (*most favored nation* was effectively conditional because subject to unilateral discretion); no monetary union was built to support the price of silver, an outcome that imposed great burdens on nations in this period; and imperial competition and the balance of power were subject to very little institutional regulation.

Of course, none of these factors is antithetical to a Gramscian-Marxist argument. It is reasonable to hypothesize that the institutional responses to crises in the international capitalist order can be more effective when they are supported by a champion or leader and that this effectiveness is further enhanced to the extent that the underlying structure of incentives is characterized by elements of coordination, rather than redistributional (prisoner's dilemma or stag hunt) elements. Furthermore, factors such as changes in domestic political cultures (e.g., the rise of the welfare state and a consequent rise in economic nationalism) could manifest themselves in many ways, particularly in altering the strategic incentives underlying relations in specific issue areas. Part of the difficulty in integrating such explanations into the author's general vision of international organization is the fact that he takes a somewhat static view of both his independent variable (the capitalist world system) and dependent variable (international organization). This is quite uncharacteristic of work in the leftist tradition, a tradition that has underscored the dynamic nature of social relations. Factoring in changes in political culture requires rendering an account of the transformation in the capitalist ideology from a classical liberal orientation characterizing the interimperial order to an embedded-liberal (i.e., Keynesian welfare consensus) orientation characterizing the free world order and beyond. The author is aware of the change, but does not carefully assess the institutional manifestations of the change across time. Similarly, the expanding functions of international organization across periods is emphasized (i.e., development, environment, public finance); but the very transformation in the nature of supranational management is neglected. International organizations in the nineteenth century tended to be institutions of limited membership (regional) with very specific functions, while the twentieth century saw the rise of institutions that had more universal memberships and general functions. It would be reasonable to draw at least some links between these two transformations: the greater challenges produce by the economic nationalism of the Keynesian welfare state required institutions with more diverse functions and comprehensive memberships. In fact, this is not antithetical to a leftist argument, because institutions evolve according to the imperatives of capitalism.

All of this suggests that the authors has found it difficult to account for the history of international organization from a singular explanatory logic, which places him in quite illustrious company. Whether one shares the author's intellectual tradition or not (I do not), one cannot escape acknowledging the importance of the contributions made by this well-crafted treatise. It is a forceful, brave, and coherent attempt at doing what is perhaps the impossible—explaining the process of international organization from a single theory of institutions. But in failing, the author has provided scholars with another substantial peg in the stepladder toward understanding the institutions that nations choose to address the problems of international relations. This book should be a part of any major library in international relations and international organization. It sets an important standard in conceptualizing and explaining from the Left the phenomenon of international organization.

Wesleyan University GIULIO M. GALLAROTTI

Domestic Choices, International Markets: Dismantling National Barriers and Liberalizing Securities Markets. By Andrew C. Sobel. Ann Arbor: University of Michigan Press, 1994. 211p. $39.50.

In sector after sector, the old guard resists change and discourages competition. Usually, the establishment prevails, but occasionally major reforms occur. Why? Andrew Sobel examines three competing explanations of the liberalization of the U.S., British, and Japanese securities markets and the relaxation of barriers to entry of foreign securities firms since the mid-1970s: (1) that changes were a response to systemic changes in the environment, (2) that the liberalization and internationalization of the securities markets were a result of foreign policy initiatives, and (3)—his preferred explanation—that changes were mainly a result of competition between organized interests within domestic political economies. In short, this volume focuses on why barriers to market access declined in all three markets and especially whether the much-heralded trend toward global interdependence and the globalization of production are the primary cause. He concludes, instead, that competition among domestic economic institutions and interests is at the core of the internationalization and liberalization of securities markets.

During the 1980s, foreign holdings of stocks in New York, London, and Tokyo increased dramatically but from a very low level. Although these three markets and their rules converged significantly during the 1980s, price trends and other measures showed less convergence among the markets than might have been predicted if market conditions and foreign policy implementation were at the heart of the changes in securities markets. Arguing that "outside-in" approaches are faced by severe roadblocks, Sobel examines in more detail an "inside-out" explanation of the shift. He suggests that "domestic contradictions and pressures provided the primary motivation for reform. The shifts would have occurred regardless of the exogenous pressures" (p. 110).

Sobel also addresses the issue of whether the United States remains an effective leader in this realm. Using interviews and reputation rankings from financial surveys he shows that U.S. leadership in financial and securities markets remains robust and persistent. He shows that there is an embedded hierarchy and asymmetrical sensitivity among markets of major countries. When U.S. markets lead, others generally follow; but the reverse is not true. Thus, the sharp decline in U.S. stocks in October 1987 reverberated around the world. By contrast the U.S. equities markets continued to climb while the Japanese market declined by more than half in the early 1990s.

Sobel concludes that "(1) domestic political-economic conflict motivated the changes in price, entry, and safety regulation; (2) the three markets remained distinctly national during the period following market openings; and (3) U.S. leadership persists despite all the discussion of decline" (p. 143). These conclusions lead Sobel to discount using technological pressures and international competitiveness arguments as a rationale to explain moves toward greater interdependence, as an impetus for policy shifts, and as a rationale for regulatory change.

Overall, this volume is a well-crafted, well-organized effort that brings social science methodology to an important, understudied area. Most of the time, Sobel's intermediate arguments are nuanced, his attention to source material is thorough, and he works diligently to generate new information and data to analyze. Sometimes, however, his trumpeting of his preferred approach becomes repetitive and a bit overly enthusiastic. He dismisses the alternatives more quickly and completely than warranted and then implies that his results may have wider utility. He has made a strong point about the relative strength of the explanations, but the other explanations remain more potent than he concedes. For example, Sobel argues strongly against giving technological developments credit for motivating regulatory changes in various countries. However, he does not explore the intricacies and the depth of the technological changes that have revolutionized trading in foreign exchange, stocks, bonds, and other securities.

Moreover, his historical survey and his methodology make the dynamic developments transforming the national and international political economy far less exciting and dramatic than they are in reality. The global securities market has spawned a dizzying array of innovations that helped fuel astonishing growth. The amount of money that routinely flows from country to country and from currency to currency now dwarfs global trade and raises major questions for policymakers. The magnitude of funds involved means that destabilizing volatility beyond the control not only of governments and their regulators but also of the large money-center banks is a possibility. The complex implications of these extraordinary developments and their potential importance for national economies and for international commerce are, for the most part, unexplored. Still, this volume takes a significant step toward understanding an important political economy realm. It is likely to motivate others to go further still.

University of Southern California JONATHAN D. ARONSON

The Sovereign State and Its Competitors: An Analysis of Systems Change. By Hendrik Spruyt. Princeton: Princeton University Press, 1995. 198p. $39.50.

A river's course has many causes: old encounters of tectonic plates, geologies laid down by ancient oceans, melting of distant snows, human abuses of adjacent land, silting of estuaries, successions of rainy seasons, and the river's own incessant grinding at its banks. The histories of states are not less complex; at best, their tellers single out a few valid causes, trace out their consequences, and neglect the rest. As a result, much useless debate about state formation concerns the relative priority of plausibly advanced causes, rather than close inspection of the particular causal chains that one analyst or another has proposed. Although now and then, he succumbs to the seduction of rival bashing, for the most part, Hendrik Spruyt sidesteps such futile attempts to weigh multiple causes. With exceptional clarity, he tells us what he wants to explain, which causes he will pursue, and why we should want to know about those causes, rather than others we might equally claim. His failures lie chiefly in his unreadiness fully to specify and verify the causal chains he emphasizes.

Spruyt wants to know why sovereign states—states based on internal hierarchy, external autonomy, and territorial exclusivity—first emerged in Europe after the eleventh century. (He neglects to state his limitation to Europe explicitly, but the existence of an internally hierarchical and externally autonomous Chinese empire for a millennium before his starting date, among other examples, makes the limitation clear; nor is it obvious that Chinese experience conforms to his explanatory model.) He claims that within any particular set of interdependent states, the dominant forms of state frequently change in spurts, in punctuations of equilibrium, separating long eras of no more than incremental alteration; that the European late medieval experience exemplifies such changes and that today's shifts in the international system parallel the earlier punctuation. He likewise argues, contrary to many neorealist readings of international history, that the organization of states and of the international system influence each other, with exogenously produced transformations of states inducing mutations in the very operation of international relations. In order to simplify the defense of his complex argument, Spruyt stages a contest among three types of state that all throve for a while in Europe between 1100 and 1648: leagues of cities, exemplified by the Hansa; city-states, exemplified by their Italian variants; and contiguous, relatively centralized states, exemplified by France. Why he excludes city-empires such as Venice and Catalonia, as well as tribute-taking empires such as Muscovy, Burgundy, the Mongols, and the Ottomans, from his contest Spruyt does not say.

Spruyt takes France as the prototype of sovereign states, the class of states that came to dominate the European system. Hence, his explanatory work falls into three familiar stages: (1) how the rise of trade in Europe after 1000 A.D. promoted the formation of city leagues, city-states, and sovereign states; (2) how sovereign states outcompeted their rivals; (3) how the mutual recognition of sovereign states drove other types out of the system or converted them into sovereign states. For the first, he takes geographically differential commercial expansion à la Pirenne and Braudel for granted, accepts as unproblematic its promotion of uneven urbanization (hence variation in the relative strength of urban elites from region to region), but then argues that within those limits, strategic bargaining among major powerholders determined locally prevalent forms of rule. In these regards, his argument directly parallels analyses of medieval history that Wim Blockmans (cited but insufficiently credited by Spruyt) has been publishing for 20 years.

What of competitive advantage? Spruyt turns to Douglass North for ideas about the superior protection of property rights, claiming that sovereign states provided greater protection and hence promoted both economic expansion and state mobilization of expansion's returns, which meant that sovereign states acted more effectively than their rivals in a variety of activities, including war. Here Spruyt suddenly turns ambivalent, unable to decide whether the selective effects of organization operated chiefly through military victories or gave states crucial competitive advantages outside of war. Did the sovereign state's standardization of measures, coinage, legal procedures plus the reduction of free riding on public goods not only serve the militarily driven fiscal needs of its rulers but somehow make their states more viable in nonmilitary ways? Spruyt's uncertain answer to this question undermines his challenge to other authors who have stressed the centrality of war, reducing his most original contribution to a specification of the means by which princes amassed superior military power via hierarchy and territorial autonomy.

How, then, did established sovereign states drive others out of the international system? Military conquest, in Spruyt's account, played a major part. But so did the selective mutual empowerment of one sovereign state by another. Two things happened. First, their organizational advantages gave rulers of sovereign states the capacity to make firmer contractual commitments vis-à-vis other states, which made them preferred partners in international agreements. Second, rulers of states organized in other ways recognized the military and contractual advantages of sovereign states and therefore acted to move their own organizations toward the sovereign type. The argument, which resembles analyses that Aristide Zolberg and other analysts of comparative politics began publishing in the 1970s, makes a good deal of sense. For some reason, however, Spruyt again turns ambivalent at this crucial point; he settles for showing that the Hansa and Italian city-states had trouble enforcing collective commitments in the face of defections by individual cities, which establishes only one or two links in the alleged causal chain. Did potential partners actually seek out alliances with French rulers *because of* their greater contractual reliability? Did sovereignty provide such visible, victory-enhancing advantages that rulers of nonsovereign states self-consciously sought to transform their own organizations in a sovereign direction? Exactly what nonmilitary advantages did emulators seek to gain from moves toward hierarchy and territorial autonomy? Was it already obvious in 1648 that the nonsovereign but enormously powerful United Provinces would lose ground to more sovereign France and England? What's the evidence for this causal account as compared to its competitors?

Spruyt has probably identified important causal mechanisms in the connected histories of European states. I hope he has, because I have invested a great deal in closely related arguments. That state systems change in spurts centered on major organizational changes, followed by longer periods of incremental change; that in such critical transformations, the organizational structures of states and the character of the international system influence each other incessantly; and that the world is currently undergoing some such transition—all coincide with my own understanding of political history. The more's the pity, then, that Hendrik Spruyt has left underspecified and largely unverified precisely those elements of his causal account which constitute his most original and controversial contributions. As a result, his closing inferences concerning current changes in the international system, conflicts within the European Community, and the unlikelihood of declining sovereignty remain provocative conjectures, rather than compelling applications of demonstrated principles.

New School for Social Research CHARLES TILLY

Mercenaries, Pirates, and Sovereigns: State-Building and Extraterritorial Violence in Early Modern Europe. By Janice E. Thomson. Princeton: Princeton University Press, 1994. 219p. $29.95.

It can be said immediately that this is an excellent book—theoretically informed and a purveyor of new data, high-powered and original, lucid, elegant, and compact. If the story it tells is in itself interesting and important, the book distinctively encourages general reflections on differential patterns of state building and their relation to war and peace.

Thomson begins by insisting that the state should not be *defined* in terms of its monopoly of violence, because long and complex historical processes were required to turn what was not initially even aspiration into reality. At the core of the book stands a useful eightfold typology of the relationships between states, markets, and violence. Particular attention is given to four neglected types of social agent: *mercenaries* are nonstate actors hired by the market, *privateers* and *pirates* are nonstate actors, (the former licensed by the state), and *filibusters* are state agents who act (or pretend to act) on their own. Further, detailed analysis is offered of the great mercantile companies of Holland and Great Britain—for long, sovereigns in their own right, especially to peoples outside Europe.

The central argument of the book is clear. States discovered over time that nonstate violence, whose low costs had initially attracted them, had disastrous unintended consequences: mercenaries tended to desert, privateers often became pirates once formal hostilities ended (thereby sowing distrust between states), while the great companies waged war not just on each other but on other European states as well. What is most noticeable about the response to these threats, according to the author, is that it was not always led by the more powerful states. Thus, Britain's desire for controls on privateering was realised as part of a larger package deal that saw Britain formally renouncing its right to interdict neutral ships in search of enemy supplies. Equally, it was the United States' Neutrality Act of 1794 that proved to be the much copied exemplar allowing for the control of mercenarism. All states were threatened, and all cooperated, first in delegitimating—and then in extirpating—nonstate violence. Thomson goes on to insist that the demand for change was not the result of domestic pressure. Rather, the complaints of other states occasioned action, in the process giving state elites a good deal of autonomy over their civil societies. This does not mean that domestic factors had no significance: the abolition of slavery undermined filibustering attempts toward the South, as did the expansion of the railroads opening new lands to the West. In general, we are given a very striking portrait of the semiconscious weakness of America's pre-Civil War state. Its inability to prevent its own employees from toppling governments has, of course, left a legacy of distrust throughout Latin America.

The author makes a powerful case against that naive realism which assumes that states have powers, rather than inquiring into the manner in which they gained them. It is particularly sobering to realise that the monopolisation of violence only takes place in the nineteenth century. Nonetheless, the argument as a whole is thoroughly statist—a fine example of sophisticated, historically guided realism. Thus, new international norms are seen as spreading insofar as, and to the extent to which, they suited state interests. At this point, the argument at times seems a little strained. One point made against the idea of British hegemony—that it provided some lead in naval affairs but none in the control of mercenaries—does not add up to much: given that British power was naval, this is just what one would expect. Furthermore, the emphasis on how a move from subject to citizen made possible a control of mercenaries is idealist and at odds with the rest of the argument. It may be right, but much more support would be needed to carry conviction. A final critical point concerns the use of secondary historical accounts. Thomson cites, without question, figures showing some pirate communities to have been very large indeed. Given the nature of the sources involved, skepticism may well be in order at this point—although the general argument, it should be noted, does not hang on this issue.

Perhaps the most important general theoretical point made by the book as a whole is that the decencies of modern life depend upon states being strong enough to territorialize and to control violence. Put negatively, there is almost nothing to be said for the trendy notion of globalization when we consider the crucial matter of violence. The extent to which our expectations in this matter have become generalized is revealed in the pariah status now accorded states that condone terrorism. But there are probably two limits to this general ethic that the author does not discuss. First, the concentration on navies and armies misses the violence perpetrated by countries whose states allow their civil societies great leeway. More particularly, Anglo-Saxon settler societies effectively wiped out indigenous populations in North America and Australasia. Second, any complete theory of the conditions for peace should face both ways as regards the benefits of territorialization. If the state's monopoly of violence brings forth our approbation, the caging of a state's elite by fervent nationalists (i.e., the insistence that the elite think only of the national interest, thereby ignoring the norms of "the society of states") can be extremely dangerous. Breakdown of international society at the end of the nineteenth century did something to cause debacle in 1914. Hence the recipe for peace must be that of strong states confident enough to participate in international society.

McGill University JOHN A. HALL

World Politics and the Evolution of War. By John J. Weltman. Baltimore: Johns Hopkins University Press, 1995. 263p. $14.95.

This book is a brief and conventional history of some major wars (and preparation therefor) from the eighteenth century to 1991, followed by inconclusive speculation on the future of warfare. Brief histories have their uses. Newcomers to the field of international politics and defence policy must start somewhere, and this book offers a starting place. The chapters are short, clearly outlined, and smoothly written. They read like good lectures from a survey course on Western civilization (and, like such courses, are written from the perspective of Europe and North America). They cite the standard secondary sources.

In a further resemblance to some of those Western civilization courses, the lecture notes may be a bit yellowed. With the exception of a phrase or two referring to the end the Cold War, virtually every word in the first eight chapters could have been written 30 years ago. An especially striking example is in the initial chapter, which sets out a realist perspective on the history of international politics. The second half of the chapter presents causes of war and consists of a summary of Waltz's *Man, the State, and War* (1954). Such a tactic is not objectionable, for the newcomer will quickly encounter references to such concepts as "the second image," but what strikes the contemporary reader is that the summary does not move beyond the words Waltz originally wrote. The more recent concept of a *democratic peace* is not mentioned at all at this point; indeed, Weltman's conclusion that there is no agreement on "the principles of domestic organization that will produce a society inclined to peaceful relations with its neighbors" (pp.

6–7) gives the impression that these words were written in ignorance of the proposition.

This introductory chapter is followed by chapters encapsulating the accepted judgments on the eighteenth century, the Napoleonic Wars, the nineteenth century, World War I, and World War II. The evocation of a survey course is reinforced by not only the frequent rhetorical questions but also the lack of a connecting theme or development of a thesis. Unlike some recent books that have looked at the effect of war on the state development (Bruce Porter's *War and the Rise of the State*, 1994) or of states on war (David Kaiser's *Politics and War*, 1990), wars are presented not so much as part of world politics but, rather, as existing in a world by themselves. Each war is examined primarily in relation to preceding wars. Thus, World War I is seen largely as a consequence of the short wars fought in Europe in the nineteenth century.

The final five chapters are both more ambitious and less substantial. Two are an attempt to summarize the nuclear weapons debate. A subject that has rarely been handled lucidly in full-length books is beyond the breadth of two short chapters. The novice will be baffled; the expert will lose patience.

The discussion of the utility of warfare since World War II suffers from lack of systematic approach and of attention to the empirical record. Weltman claims, "In only a few instances have governments successfully secured decisive advantage" (p. 191). But his evidence comes from a biased selection. Even the simplest empirical analysis suggests that since 1945, 6 out of 20 wars have secured decisive advantage for the initiator (the Vietnam, Six Day, Bangladesh, Yom Kippur/Ramadan, Falklands, and Persian Gulf Wars). Weltman counts several of these recent wars as indecisive only because he views them from the perspective of the United States or its allies. The 1973 war was inconclusive for Israel, perhaps, but arguably not for Egypt; the Vietnam War accomplished nothing from the U.S. point of view, but was quite successful for North Vietnam.

A case can be made for avoiding too great an emphasis on empirical findings in writing for newcomers, but such restraint does not license an author to offer questionable empirical judgments as obiter dicta. Yet here we read, without any supporting evidence, that empires do not collapse without war (Manchu China?), that there has been no direct armed conflict between the great powers (China and the USSR along the Ussuri River?), and that there have been no interstate wars in Europe since 1945 (Hungary 1956? Cyprus 1974?) (pp. 169, 200). A reader encountering such surprising judgments expects an accompanying argument.

The final chapter speculates on the role of war in the coming years. Like so much futurology, the observations are either truisms ("The world may not be an especially peaceful place, . . .") or equivocation (the world will more peaceful because of the lack of ideology, but a new ideology could arise) (pp. 221–22). Every statement is so hedged with qualifications that almost any conceivable future could count as confirmation.

Weltman appears to believe that war will persist indefinitely. He warns that "we should not expect the world after the cold war will at last be a world at peace" (p. 19). This view has been challenged by those who have embraced the theory of a democratic peace. Given the seriousness of the challenge, his treatment of democratic peace is surprisingly offhand. Instead of a systematic examination, he misrepresents it and then belittles it with throwaway lines—that democratic America nevertheless went to war against Iraq in 1991 or, more astonishingly, that Hitler's coming to power through "the electoral process should cast grave doubt on the claim that democratic forces are inherently pacific" (pp. 120, 219). In these passages, he is attacking a theory of his own devising. The claim is not that democracies do not fight; even President Clinton got it right in his State of the Union address when he said, "Democracies don't attack each other."

When the precise proposition of democratic peace theory is examined in the final chapter, it is rapidly dismissed by raising questions to which its defenders have already offered answers—that there have been too few democracies and too short a time period to judge and that, alternatively, it was the U.S. security umbrella that prevented war. A one-paragraph treatment of what has become one of the most debated propositions about the future of war is less than adequate, even in an introductory book. Whatever its weaknesses, the theory of the democratic peace has the virtue of being a clear and testable proposition. This book would have benefitted from a similar proposition as its organizing principle.

Western Washington University DAVID W. ZIEGLER

Feminism and International Relations: Towards a Political Economy of Gender in Interstate and Non-Governmental Institutions. By Sandra Whitworth. New York: St. Martin's, 1994. 184p. $39.95.

Sandra Whitworth's *Feminism and International Relations* is a welcome addition to a new and fast-growing literature that is beginning to incorporate gender analysis into international relations theory. Using the International Labor Organization (ILO) and the International Planned Parenthood Federation (IPPF) as case studies, it makes a compelling case in support of the claim that an analysis of changes in their attitude toward gender relations over time is necessary for a complete understanding of the evolving policies of those institutions. Such empirical investigations are a step forward in testing some of the theoretical claims made by recent feminist approaches to international relations. Even if Whitworth's assertion that "the 'next step' of IR theory will not be one which is merely 'critical', but one which is *both* critical *and* feminist" (p. 56) might be somewhat overdrawn, certainly *Feminism and International Relations* provides a convincing argument for the importance of incorporating feminist analysis into the new critical literatures in international relations.

Feminism and International Relations begins by providing a critical review of a variety of recent feminist approaches to international relations. Although Whitworth's categorization of these authors is somewhat oversimplified, it does provide a useful starting-point for students of international relations, unfamiliar with these writings. Whitworth's preferred approach—the one that informs her study—is what she terms *critical/feminist international relations*. It is grounded in the work of critical international theorists (e.g., Robert Cox) and socialist feminists (e.g., Cynthia Enloe and Maria Mies). Like most contemporary feminists, Whitworth defines gender as socially constructed: she uses her cases to demonstrate how unequal gender relations are formed and how they change over time.

Before applying her critical/feminist approach to the study of international organizations, Whitworth addresses a more general and fundamental question as to whether it is possible to theorize about gender from within existing international relations theory. Using an amended version of Michael Banks's tricotomized overview of the field, she explores the potential for gender analysis in realism, pluralism, and critical theory. While certain regime theorists would probably resist Whitworth's categorization of them as realists, Whitworth concludes that classical realism and regime theory are inhospitable territory for gender analysis because of their ontological commitment to states and states*men* (p. 55). Similarly, pluralism, represented by the *world society* approach of John Burton and the World Order Modelling Project, cannot adequately incorporate gender due to their ahistoricism and, according to Whitworth, their undertheorizing of social and political structures.

Rejecting postmodernism because of its tendency toward political paralysis, Whitworth sees the most potential for gender analysis in international theory in the Gramscian historical materialist tradition. This approach permits an examination of her central question—how individuals, operating in particular historical and material circumstances create institutions. She is especially concerned with the way in which women and men engage with each other and the institutions within which they are situated in order to create, maintain, and change particular understandings of gender. Put in the context of the agent-structure debate in international relations theory, Whitworth's analysis sees structure and agency as mutually constitutive of one another.

Applied to the ILO, this analysis reveals that whereas the ILO previously treated male workers as the norm and women, by virtue of their reproductive function, as special cases, it is now beginning to reflect struggles for gender equality. Conversely, in the past, the IPPF rendered women invisible, although it, too, has made similar progress by beginning to recognize the importance of women's equality and reproductive freedom over its previous commitment to family values.

Discussion of the cases are relatively short: issues of class and race that are central to any analysis of women across cultures are not raised until the concluding chapter and then only briefly. Similarly, the analysis of the potential of international theory for gender analysis is tantalizing short: critiques of some approaches are derived exclusively from a brief review of some secondary sources.

Yet brevity can also be a virtue: *Feminism and International Relations* makes a concise but significant contribution to our understanding of the construction of gender relations over time and its effect on international institutions. Its accessibility and straightforward prose, as well as its skillful integration of feminist theory with critical approaches to international relations, are to be commended. Besides its importance for furthering the effort to construct gendered approaches to international theory, this is a text that should be read by students of international political economy and international organization, as well as those concerned with recent debates on population and employment. Since one of its central concerns is the effects of gender construction on the lives of men, as well as women, *Feminism and International Relations* offers some important new insights for us all.

University of Southern California J. ANN TICKNER

Behavior, Culture, and Conflict in World Politics. Edited by William Zimmerman and Harold K. Jacobson. Ann Arbor: University of Michigan Press, 1993. 336p. $49.50.

Conflict is a pervasive aspect of social life. Individuals—often organized as groups—have interests and act to further these interests. When the goals of two or more actors cannot be achieved simultaneously, a conflict of interests results. Each participant to such a dispute has two incentives, which may be at odds. Each desires to further its interests to the greatest degree possible, but each also wishes to settle the dispute as cheaply as possible. The analysis of the processes by which individuals and groups resolve conflict is perhaps the central question in political science. Determining how competing actors interact, showing how institutional structures (formal and informal) influence conflict resolution, and understanding how cultural beliefs and norms affect the behavior of conflicting parties are all necessary for explaining how political actors balance their competing and cooperative interests in resolving their disputes. Nowhere is the importance of the study of such phenomena clearer than in the field of world politics. The actors in this arena often pursue their interests through the use of military force, which can result in the devastation of vast resources and the destruction of millions of lives. The passing of the Cold War has not ended interstate war, nor has it reduced the destructive potential of modern weaponry. Understanding interstate conflict remains important for reasons beyond historical curiosity.

Arguably, the advances in our understanding of international conflict made over the past few decades by the faculty of the University of Michigan are greater than those made by any other collection of scholars. This book is, in many ways, a retrospective of these contributions. Each of the selections is authored (or coauthored) by a senior faculty member at Michigan. The collection contains works by economists, philosophers, social psychologists, historians, and ecologists, as well as political scientists. Each piece is representative of the work conducted in a particular area, and the compilation provides an overview of the breadth of research conducted on various aspects of international conflict. The reader is introduced to research accounting for interstate war, work investigating international economic conflict and cooperation, studies showing how different cultural conceptualizations of conflict affect the course of international disputes, and research into the biological and psychological factors affecting conflict behavior. None of the contributions is overly technical, and each selection is quite accessible to nonspecialists. The goal is to offer the reader an introduction to a broad range of research and to show how work from a variety of approaches can contribute to the development of a general understanding of international conflict.

Efforts of this type always face the problem of conveying to the reader that the selections are contributing to a common understanding. This volume is no exception. Though all are dealing with conflict in world politics, the specific questions addressed, the basic concepts and assumptions adopted, and the methodological approaches utilized are so varied that the reader is challenged to perceive a common theme. Jacobson and Zimmerman make a concerted effort to unify the volume in their concluding chapter, but the common elements are too general for the whole to contribute much more than the sum of its parts. The other weakness of the book is that little effort was made to push the boundaries of our knowledge. I was familiar with the arguments and research presented by the political scientists and economists, and I presume that specialists in the other fields represented would be familiar with contributions in those areas.

That said, I shall note that the book does make a contribution. The work represented is among the most important scholarship addressing international conflict. One can learn much from research in other disciplines, and this book provides easy access to major arguments in a variety of fields. It is worth reading by anyone interested in international conflict and will be particularly useful for students trying to come to terms with the range of research into the topic. Although it is difficult to identify a common theme for the entire volume, it is possible to detect areas in which pairs of traditions overlap; and one can see how scholarship from other fields can increase one's understanding of one's own field. On this level, the book succeeds.

Rice University T. CLIFTON MORGAN

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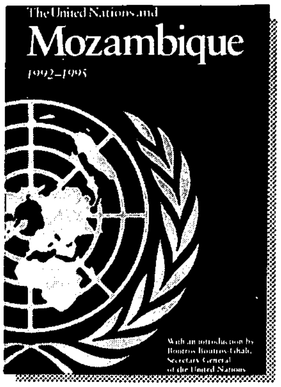

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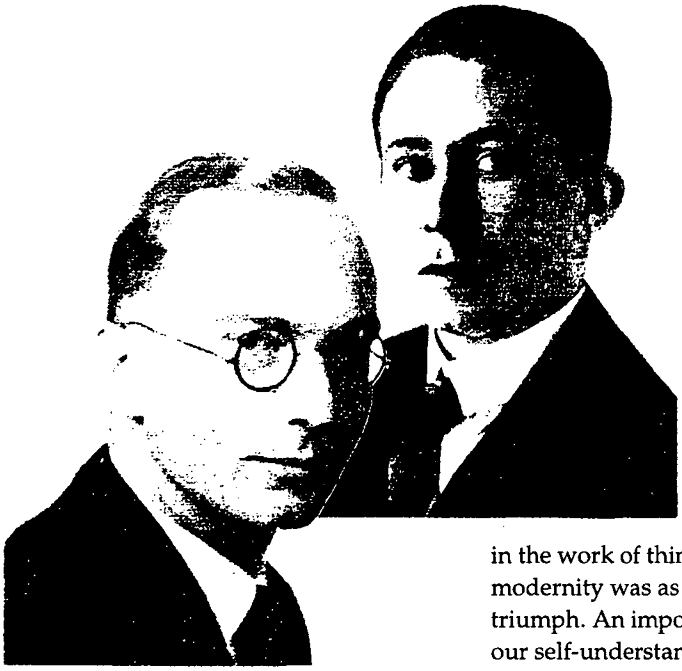

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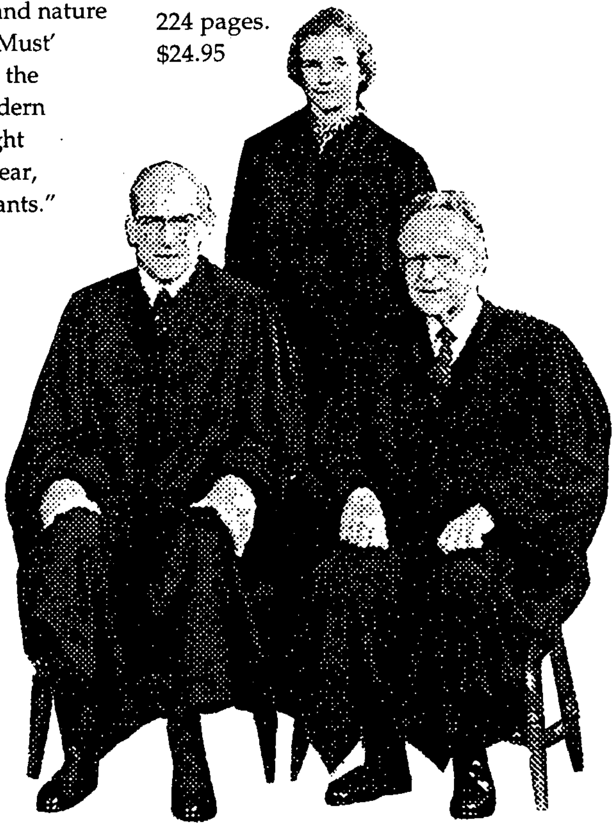

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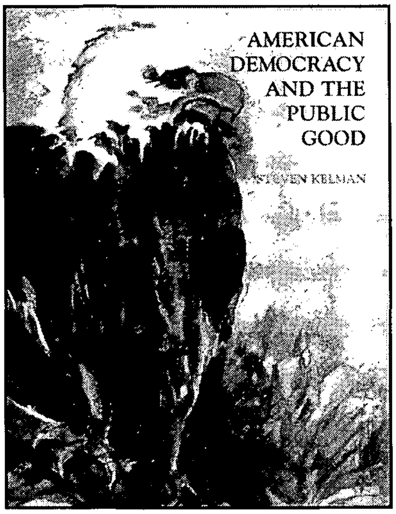

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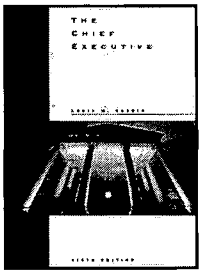

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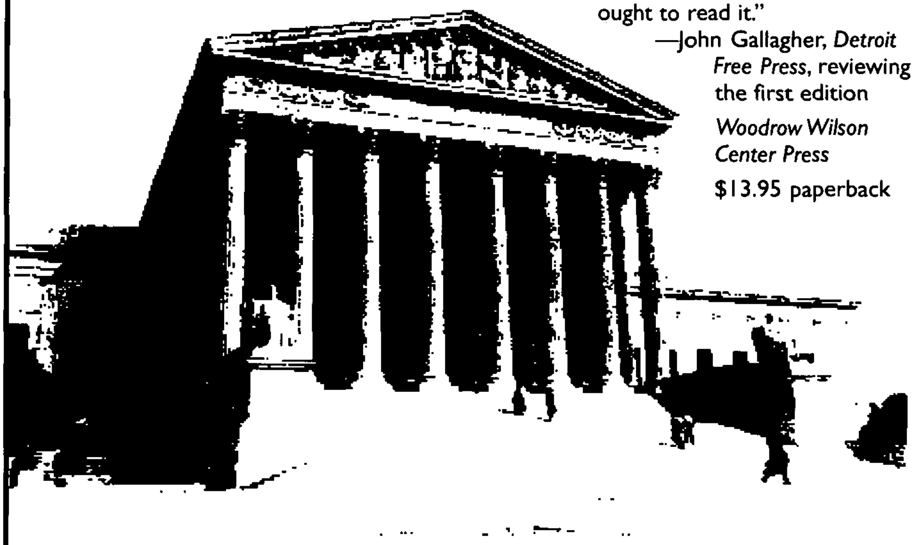

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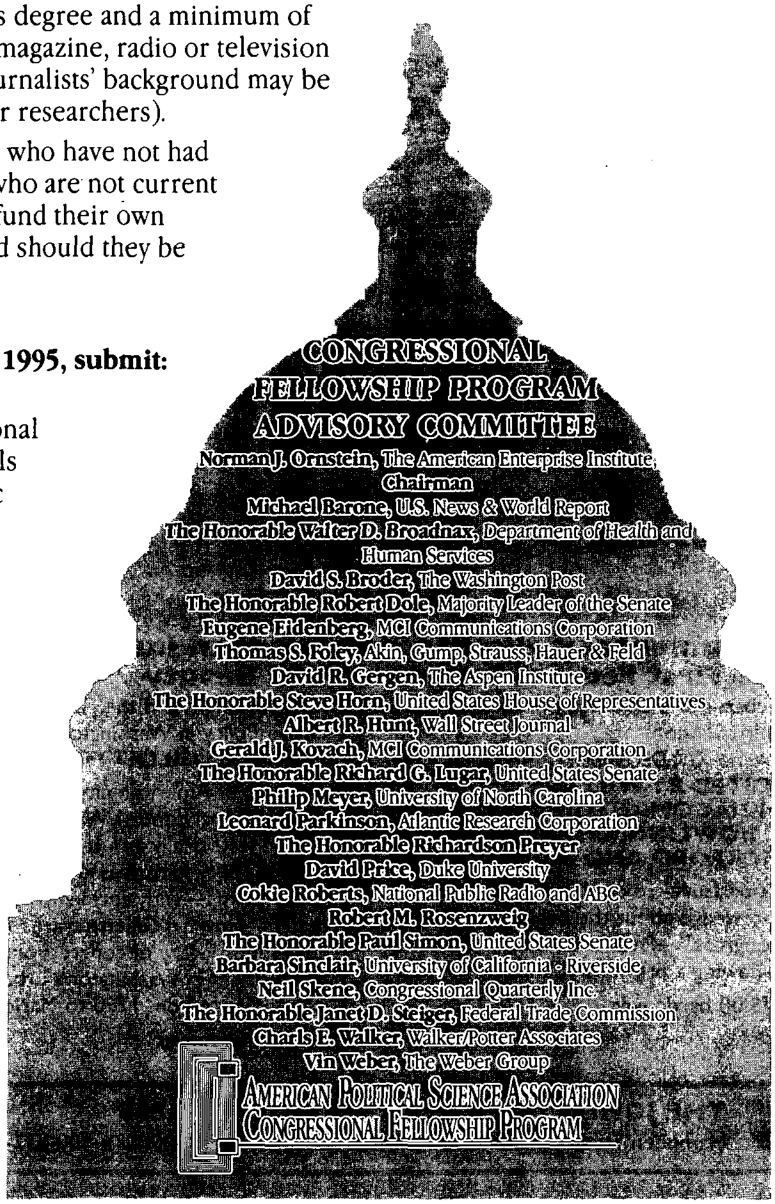

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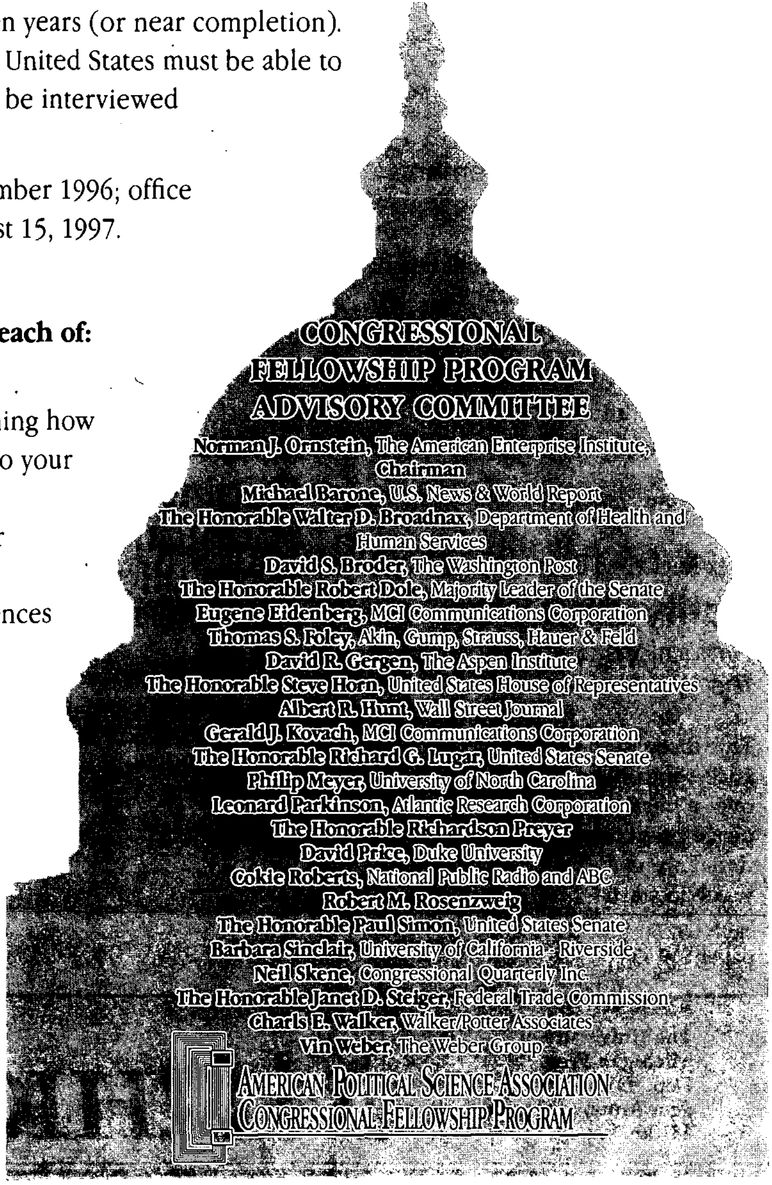

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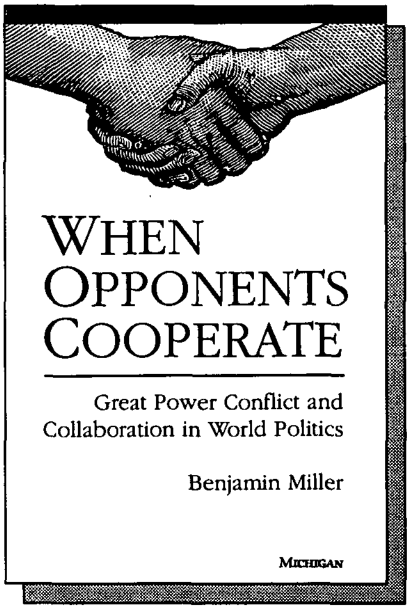

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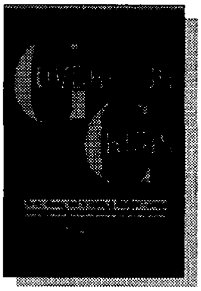

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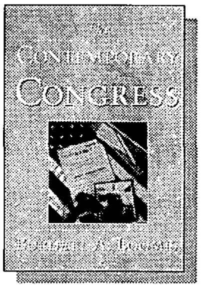

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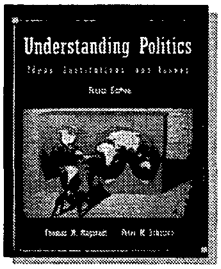

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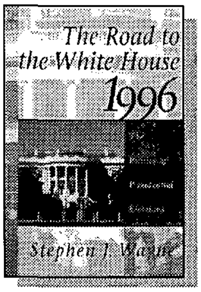

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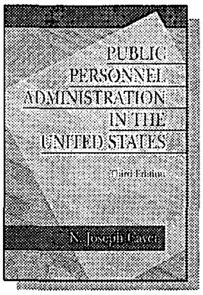

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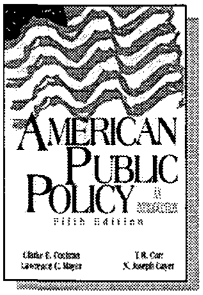

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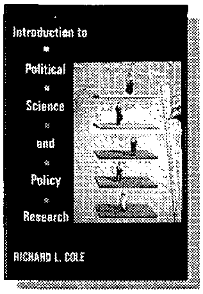

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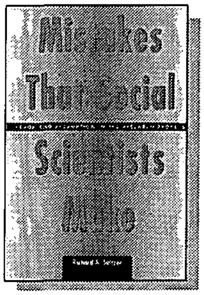

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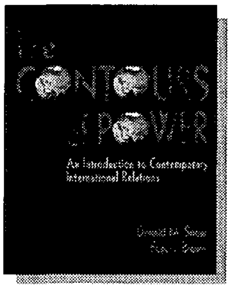

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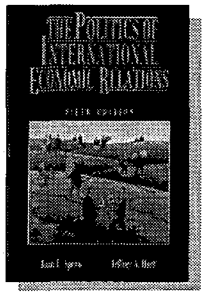

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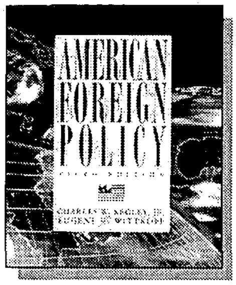

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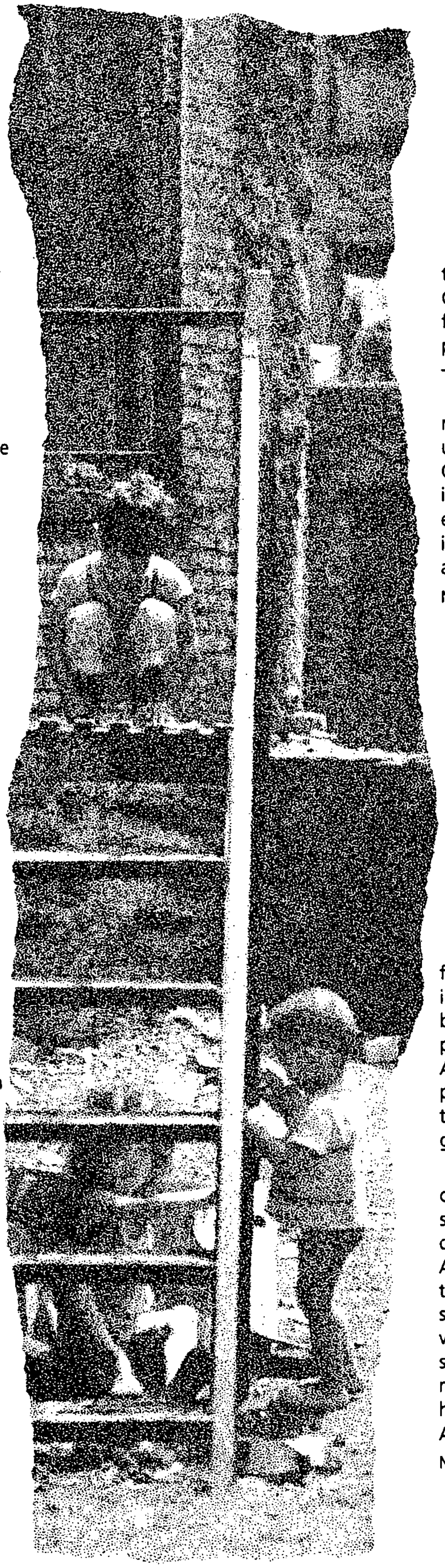

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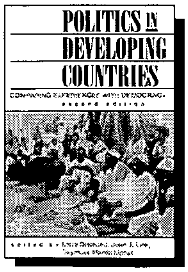

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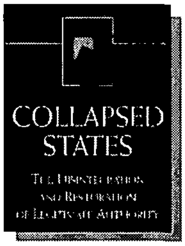

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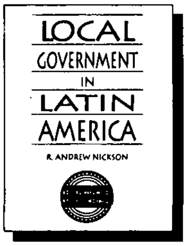

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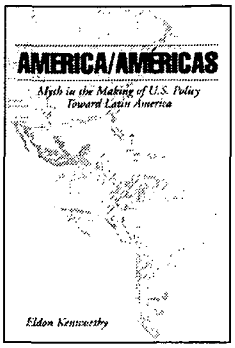

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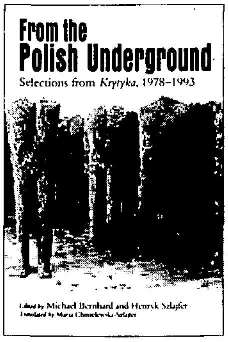
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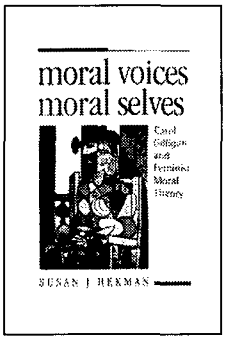

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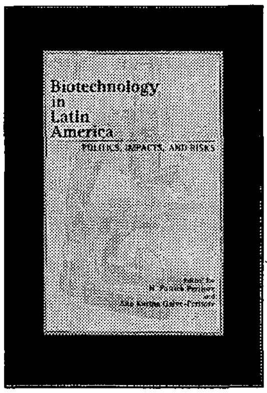

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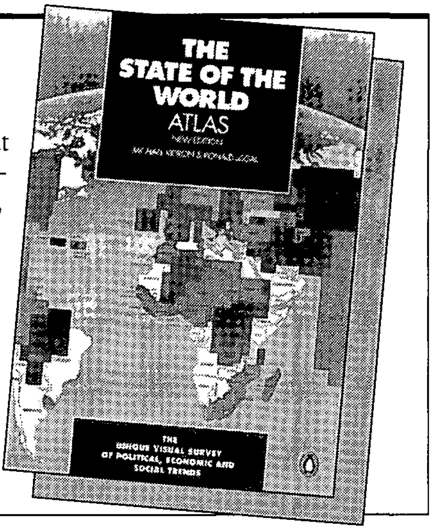
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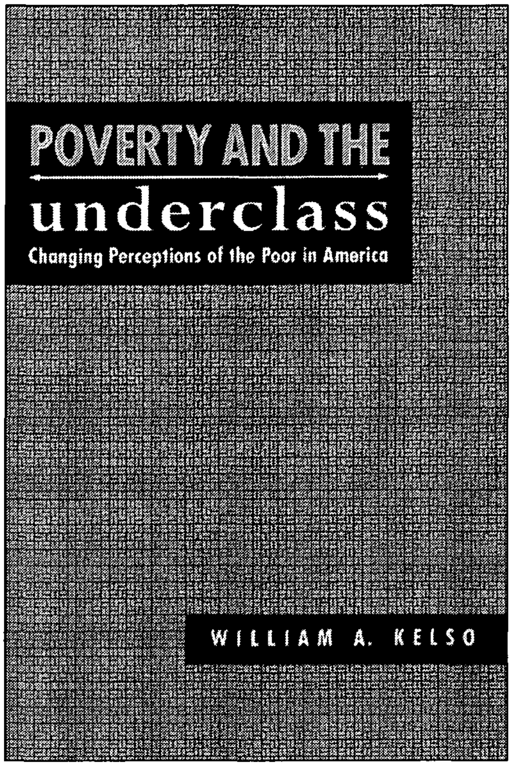

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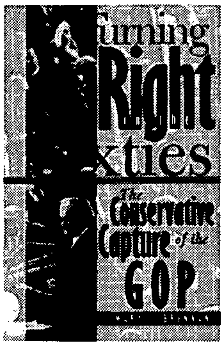

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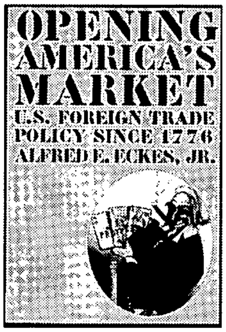

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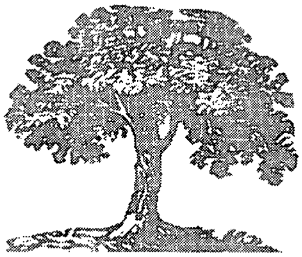

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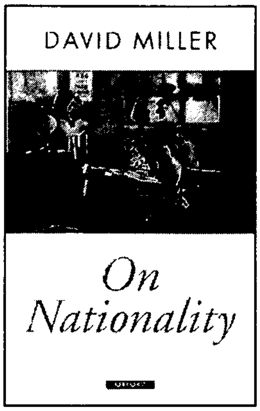

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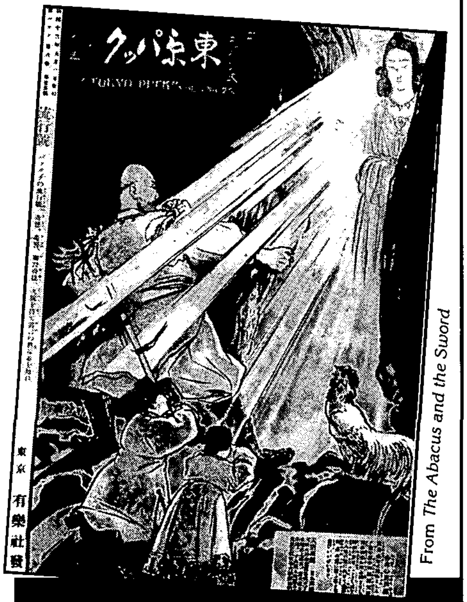

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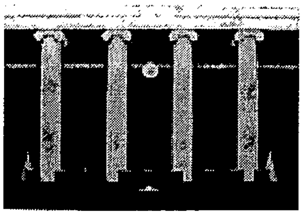

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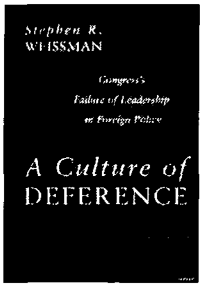

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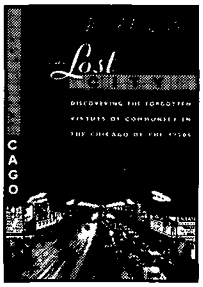

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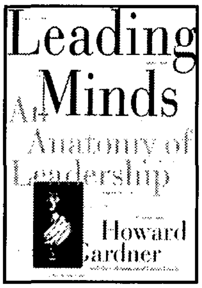

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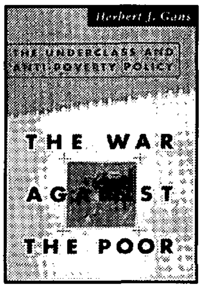

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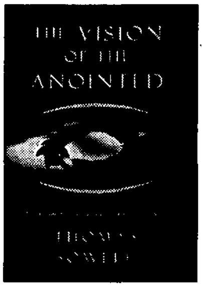

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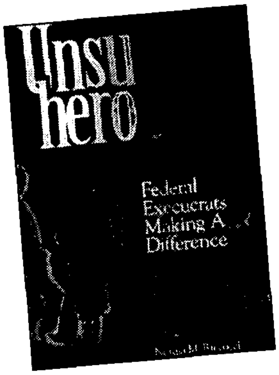

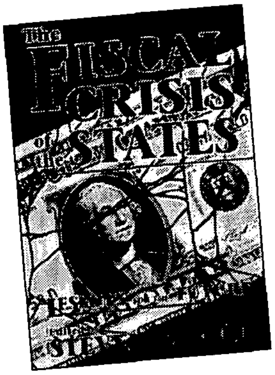

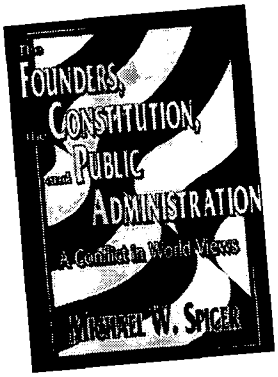
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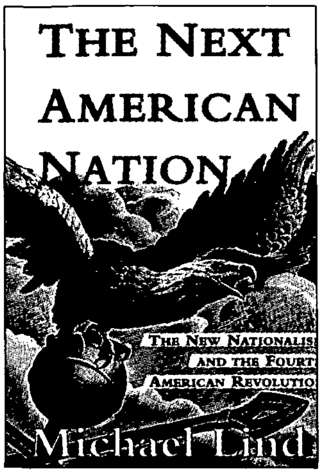

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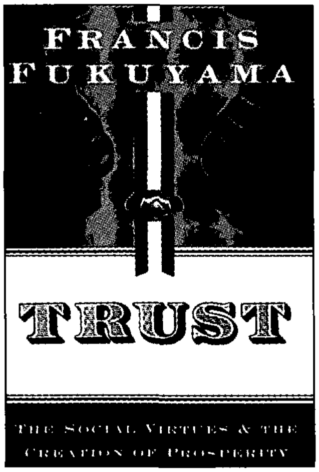

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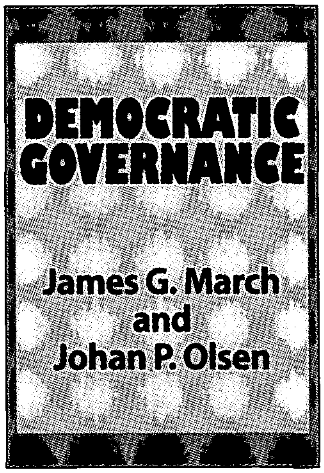

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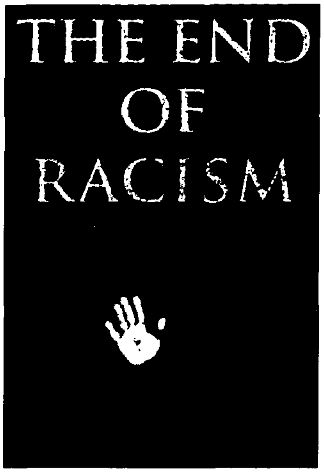

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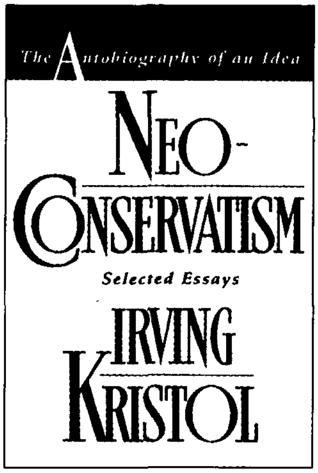

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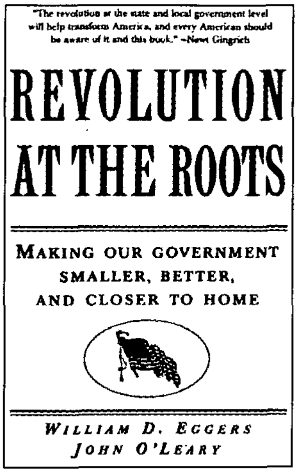
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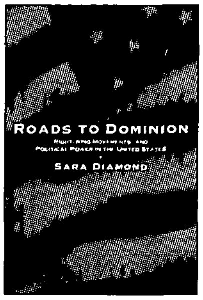
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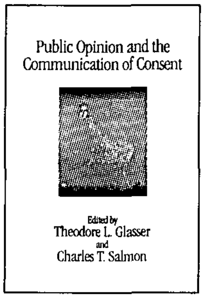
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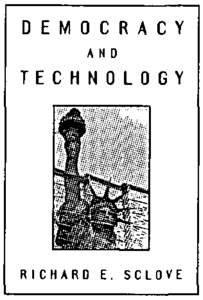
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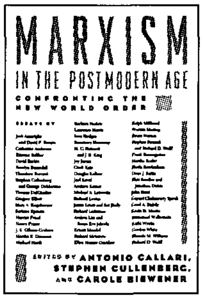
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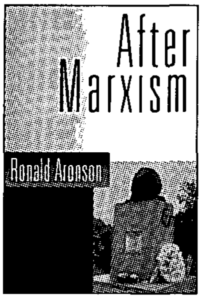
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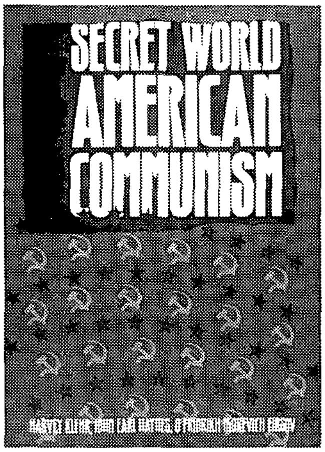

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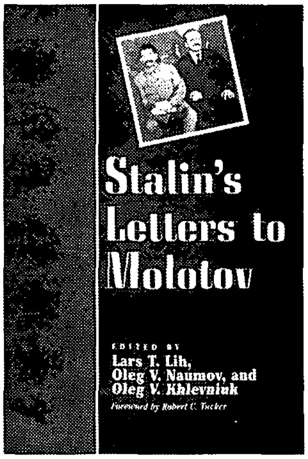

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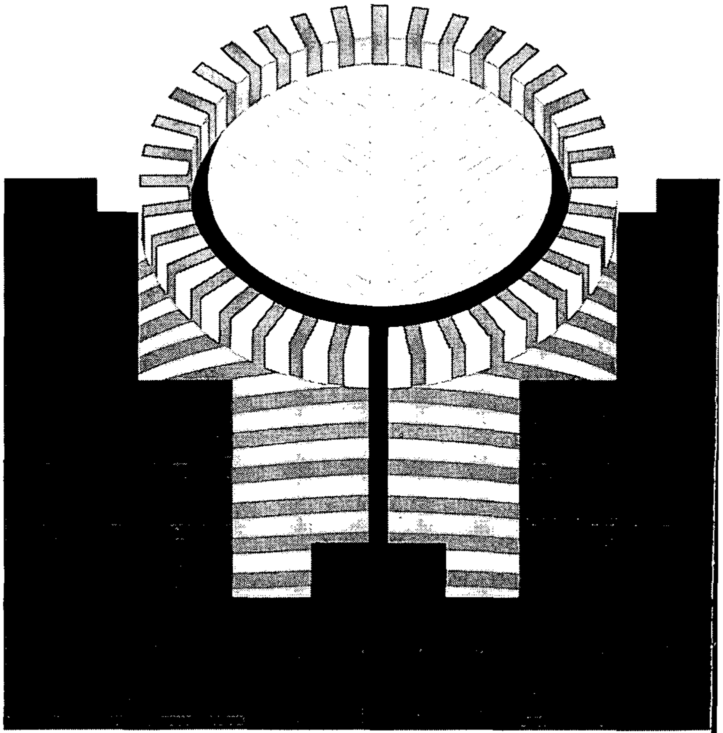

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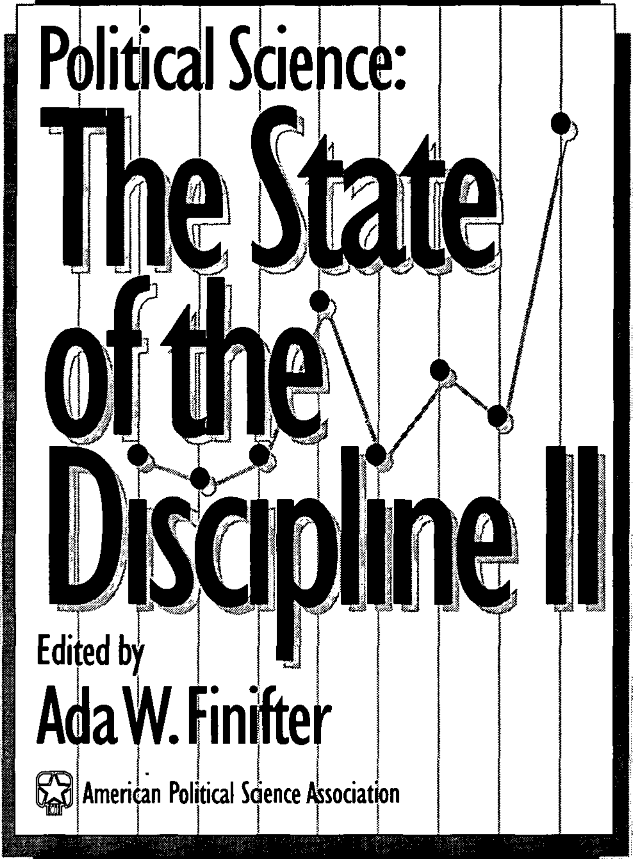

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Table of Contents

ARTICLES

Dynamic Representation
James A. Stimson, Michael B. MacKuen, and Robert S. Erikson 543

Campaign Contributions and Access
David Austen-Smith 566

A Grammar of Institutions
Sue E. S. Crawford and Elinor Ostrom 582

The Advantage of Being Moderately Cooperative
Tomonori Morikawa, John M. Orbell, and Audun S. Runde 601

Thick-Skinned Liberalism: Redefining Civility
Richard C. Sinopoli 612

Mary Astell (1666–1731), Critic of Locke
Patricia Springborg 621

What To Do (and Not to Do) with Time-Series–Cross-Section Data in Comparative Politics
Nathaniel Beck and Jonathan N. Katz 634

Coalition Termination and the Strategic Timing of Parliamentary Elections
Arthur Lupia and Kaare Strøm 648

RESEARCH NOTES

Waltz, Durkheim, and International Relations: The International System as an Abnormal Form
John Barkdull 669

The Consequences of Negotiated Settlements in Civil Wars, 1945–1993
Roy Licklider 681

Issue Fluidity on the U.S. Supreme Court
Kevin T. McGuire and Barbara Palmer 691

CONTROVERSIES

The Empirical Evidence for Citizen Information and a Local Market for Public Goods
David Lowery, W. E. Lyons, and Ruth Hoogland DeHoog; Paul Teske, Mark Schneider, Michael Mintrom, and Samuel Best 705

The Enactment of Mothers' Pensions: Civic Mobilization and Agenda Setting or Benefits of the Ballot?
Cheryl Logan Sparks and Peter R. Walniuk; Theda Skocpol 710

NOTES FROM THE MANAGING EDITOR 731

BOOK REVIEWS

Political Theory 738

American Politics 756

Comparative Politics 773

International Relations 794